Title
Essays on Development Economics

Author
Vera Cossio, Diego

Publication Date
2018

UNIVERSITY OF CALIFORNIA SAN DIEGO

Essays on Development Economics

A dissertation submitted in partial satisfaction of the
requirements for the degree of Doctor of Philosophy

in

Economics

by

Diego Alejandro Vera Cossio

Committee in charge:

Professor Prashant Bharadwaj, Chair
Professor Gordon Dahl
Professor Craig McIntosh
Professor Karthik Muralidharan
Professor Krislert Samphantharak

2018

The Dissertation of Diego Alejandro Vera Cossio is approved and is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California San Diego

2018

DEDICATION

To my beloved mom, Maria Elena, for her constant motivation and encouragement to pursue my dreams. She may no longer be with us, but I am sure she is enjoying this accomplishment from Heaven. To my brother, Horacio, for always filling in for me during this long time away from home. To my dad, Walter, for dedicating his life to provide us with opportunities. To my aunt, Maria Luisa, for providing her loving support. To Ashley, for being my unconditional partner in this journey.

EPIGRAPH

Poverty is not just a lack of money;
it is not having the capability
to realize one’s full potential as a human being.

Amartya Sen

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

ACKNOWLEDGEMENTS

I would like to thank Professor Prashant Bharadwaj for his support as the chair of my committee. His commitment to my work and his encouragement were fundamental. I feel lucky for having had him as an advisor. I am also very grateful to Gordon Dahl, Craig McIntosh, Karthik Muralidharan and Krislert Samphantharak for always willing to spend time discussing my research.

I would like to thank my peers, Desmond Ang and Mauricio Romero, for valuable feedback and providing great company during the job-market process. Similarly, I would like to acknowledge Patrick Bloom, Dodge Cahan, Mitch Downey, Kilian Heilman, Claudio Labanca, Bruno Lopez, Julian Martinez, Pablo Ruiz and Gonzalo Valdez for making my experience at UCSD a pleasant one.

I would also like to acknowledge my co-authors, Robert Townsend and Emily Breza.

Chapter 1, is currently being prepared for submission for publication of the material. Vera Cossio, Diego ”Targeting credit through community members”.

Chapter 2, is currently being prepared for submission for publication of the material. Vera Cossio, Diego ”Dependence or constraints? Cash transfers and female labor supply”.

Chapter 3, is currently being prepared for submission for publication of the material. Breza, Emily; Townsend, Robert; Vera Cossio, Diego ”Access to credit and productivity: evidence from Thai Villages”. The dissertation author has contributed significantly to the collaborative research.

VITA

2008	Bachelor of Arts, Universidad Catolica Boliviana San Pablo. La Paz, Bolivia.
2009–2010	Research Assistant, Institute of Advanced Development Studies INESAD. La Paz, Bolivia.
2011	Master of Arts in Economics. Universidad de Chile. Santiago, Chile.
2012-2013	Research Fellow, Inter-American Development Bank. Washington, District of Columbia. USA
2018	Doctor of Philosophy in Economics. University of California, San Diego.

FIELDS OF STUDY

Major Field: Development Economics

ABSTRACT OF THE DISSERTATION

Essays on Development Economics

by

Diego Alejandro Vera Cossio

Doctor of Philosophy in Economics

University of California San Diego, 2018

Professor Prashant Bharadwaj, Chair

A fundamental concern in development economics is the presence of institutional and labor market failures that interact with frictions in financial markets, which may prevent economic growth. This dissertation studies the importance of these interactions in a series of three papers. Chapter 1 studies the extent to which by allowing grassroots organizations–as opposed to banks–to allocate publicly-funded credit, it is possible to overcome existing financial frictions and deliver resources the community members who need it the most: poor, high-productivity households. Using a long panel dataset I find evidence of misallocation: credit was provided to households with poor credit history, which were richer and less productive than non-borrowers. Instead, resources were delivered to households with connections to local political leaders. The

results highlight the limitation of community-based approaches to allocating public resources in developing countries. Chapter 2 shows that a cash-transfer program targeted to children in Bolivian public schools boosted employment among mothers of beneficiary children by providing extra-liquidity in a context of fixed costs to work. Chapter 3 exploits rich data from Thailand to show that estimates of total factor productivity can be used to predict business success in the aftermath of credit-expansion programs.

Chapter 1

Targeting Credit through Community Members

Abstract

Delegating the allocation of public resources to community members is an increasingly popular form of delivering public resources in developing countries. However, this approach is associated with the tradeoff between improved information about potential beneficiaries and favoritism towards local elites, which could be strengthened in the context of credit. Unlike targeting cash transfers to the poor, the optimal targeting of credit is a more complex problem involving issues of productivity, repayment, and market responses: This paper analyzes this problem using a large-scale lending program, the Thai Million Baht Credit Fund, which decentralizes the allocation of loans to an elected group of community members, and provides three main results. First, exploiting a long and detailed panel, I recover pre-program structural estimates of household productivity and find that resources from the program were not allocated to high-productivity, poor households, which is inconsistent with poverty and productive efficiency as targeting criteria. Second, using socioeconomic networks data, I show that actual targeting is strongly driven by connections to village elites and is related to lower program profitability, which suggests favoritism as a reason for mistargeting. Finally, I exploit quasi-experimental variation in the rollout of the program and uncover evidence that, in general equilibrium, informal credit markets compensate for targeting distortions by redirecting credit towards unconnected

households, albeit at higher interest rates than those provided by the program. The results highlight the limitations of community-driven approaches to program delivery and the role of markets in attenuating potential targeting errors.

1.1 Introduction

Community-driven development approaches to delivering public resources have gained increasing attention from academics and policy makers around the world. In developing countries, a number of social programs such as public works or cash transfer programs rely on community members for their implementation or monitoring.[1] One of the foundations of this approach is the idea that community members, as opposed to traditional policy makers, have better information to identify local needs. In the context of credit, delegating the allocation of resources to community members may lead to more accurate identification of potential borrowers and may fulfill the promise that was only partially materialized by traditional microfinance: providing affordable credit to poor, high-productivity households.[2]

One important class of community-based policies to expand access to credit is that of government infusions of resources into villages for the establishment of local credit funds which are managed by elected groups of community members.[3] The economic rationales for this approach include the reduction of intermediation and administrative costs as well as the benefit

[1]See for example Mansuri and Rao (2004) for a review in the case of community-based approaches to infrastructure projects. Community based targeting of cash transfers has been studied by Alatas et al. (2012), and participatory rankings among community members have been used in graduation programs (Banerjee et al., 2015), and other programs that involved the delivery of cash transfers to the ultra poor (Bandiera et al., 2017).

[2]Uptake of credit in recent microcredit interventions has been low, due to, among other reasons, high interest rates and the difficulty of identifying high-productivity borrowers (Banerjee et al., 2015; Crpon et al., 2015; Banerjee et al., 2015). Reviews from either a policy or an academic perspective regarding the challenges of microfinance are provided by World Bank (2008); Armendáriz de Aghion and Murdoch (2004); Banerjee and Duflo (2010); Morduch (1999); Karlan and Morduch (2010).

[3]Broadly, community-based credit approaches consist of fostering local credit funds to be managed by community members. A clear example is the Million Baht program in Thailand (Kaboski and Townsend, 2012) and the Integrated Rural Development Program in India (Bardhan and Mookherjee, 2006b). While self-funded village credit groups are a growing research topic in the literature (see Deininger (2013); Greaney et al. (2016); Ksoll et al. (2016); Karlan et al. (2017), among others), there are other types of government-funded programs with a community based approach around the world such as the Andhra Pradesh Rural Poverty Reduction Project in India, and the Rural Financial Institutions Programme in Uttar Pradesh.

from information available to community members, which is costly to obtain by policy makers. On the other hand, community members may engage in favoritism towards politically connected households (Bardhan and Mookherjee, 2005). This tension is particularly salient in cases in which community members disperse public funds based on criteria that are hard to observe (unlike poverty targeting) and subject to moral hazard, as is the case with credit markets. Thus, whether the allocation of resources is consistent with poverty, productive efficiency or favoritism as targeting criteria is an empirical question. While the ability of community members to identify profitable households has been documented (Hussam et al., 2017), little is known regarding the effective use of this information when community members themselves are in charge of the allocation of public credit. Moreover, although the use of pre-program data has been essential for the empirical analysis of community-based approaches to delivering resources to the needy (Alatas et al., 2012), other studies analyzing how local leaders allocate productive resources are based on post-program measures of productivity which are likely to be affected by the program itself (Bardhan and Mookherjee, 2006b; Basurto et al., 2017). In addition, previous studies have focused only on understanding how community members allocate resources but have ignored the role of markets in reallocating resources, which may attenuate potential targeting errors.

This paper empirically assesses these issues in the context of the Thai Million Baht Village Fund (MBVF) which is one of the largest community-based credit programs. Between 2001 and 2002, the government donated resources to over 90% of rural villages for the creation of local credit funds, which represented, on average, a 25% increase in the available funds for credit in each village. These funds were fully managed by elected village committees made up of community members, who decided who obtained credit and under what loan conditions.[4] This paper reports results from three empirical exercises: first, using a long panel, I structurally

[4]The importance of this program and the fundamental tradeoffs in the allocation of productive resources have been of interest in the literature, but there are both unanswered questions and methodological limitations to existing studies. Kaboski and Townsend (2012) and Kaboski and Townsend (2011) have documented the effects of the MBVF on several household outcomes and the cost-effectiveness of the program. Breza et al. (2017) analyze whether baseline productivity explains heterogeneity in the effects of the program on investment and income growth but do not explore the mechanisms behind the allocation of resources from the program. Thus, what the program's *de facto* targeting criterion was–poverty reduction, productive efficiency, or favoritism–is yet unknown.

estimate a household production function and use the estimated factor elasticities to recover *pre-program* estimates of household total factor productivity.[5] I combine these estimates with baseline per-capita consumption data to test: *(i)* whether village committee members delivered credit to poor, high-productivity households, and *(ii)* whether offering credit to villagers based on alternative targeting criteria (i.e., means-testing and a baseline repayment probabilities) would have delivered credit to poor, high-productivity households. Second, I combine detailed data on pre-program socioeconomic networks with data about loan characteristics and repayment to test for favoritism towards households with connections to the local elite. Third, I use quasi-experimental variation in the rollout of the program to test for within-village general equilibrium responses in credit markets, which could lead to program spillovers to households with limited access to credit from the program.

First, I find that the program does not target poor, high-productivity households and that, in terms of poverty and productive efficiency, the program is outperformed by alternative targeting criteria. In practice, the allocation of loans was regressive and productively inefficient: the distribution of baseline per-capita consumption corresponding to program beneficiaries first-order stochastically dominated that of non-beneficiaries. Moreover, only 40% of high-productivity households (top 25% of the productivity distribution) borrowed from the program, and , on average, program borrowers had lower baseline productivity than non-borrowers. This allocation was not consistent neither with concerns regarding equity nor repayment. By comparing program borrowers to households that would have been eligible under an alternative targeting criterion based on baseline wealth rankings (i.e., means testing), I find that, on average, the means-testing criterion would have targeted the poorest households without sacrificing productivity. Furthermore, by comparing program borrowers to households that would have been eligible under an alternative targeting criterion based on baseline repayment probabilities (i.e., repayment score), I find that 38% of households who received credit from the program would

[5] Concretely, I exploit data on households' financial statements, in particular balance sheets, to measure capital as the value of the stock of total fixed assets for each household. The financial accounts data was compiled by Samphantharak and Townsend (2010).

have been ineligible under the repayment-score criterion. On average, these households were 12% less productive than households who did not borrowed from the program but exhibited high repayment probabilities. Reallocating program resources across these groups would have led to an average productivity gain of 4.5%, at no cost in terms of baseline per-capita consumption.

Second, while neither poverty targeting nor productive efficiency were the relevant allocation criteria, subsidized credit was disproportionately allocated to households with socioeconomic connections to the local elite. Combining socioeconomic networks data and data on baseline membership in the village council (the highest political authority in each village), I classify households as connected with the elite if they *i)* are members of the village council, *ii)* are first-order kin of the local elite, or *iii)* had direct pre-program socioeconomic ties to the local elite. I find that connected households are 20 percentage points more likely to obtain credit from the program than unconnected households. Connected households were not poorer or more productive than unconnected households, and yet they obtained more credit. Moreover, connected households already had access to institutional credit before the program and had similar baseline delinquency rates. While the correlation between program participation and connection to local elites falls by 45% after controlling for total number of connections in the village, demographic characteristics, business orientation, and credit history, connected households were still 10 percentage points more likely to obtain credit from the program. Thus, the slanted allocation towards connected households was only partially explained by improvements in information regarding borrower characteristics.

I find evidence of favoritism towards connected households with implications for program profitability. Connected households were favored with low initial interest rates leading to *ex post* lower internal rates of return for the program. A cross-section sample of loans corresponding to 344 households who borrowed both from the program and privately funded local credit groups allows me to compare loan performance across different lenders for the same household and control for unobserved borrower characteristics.[6] I test for favoritism by analyzing whether

[6]These groups constitute quasi-formal sources of credit. They include production credit groups and women's,

connected households obtain more favorable loan conditions in the case of program loans compared to loans from private credit groups and comparing these differences to those for unconnected households. The results show that program loans to connected households were granted at lower initial interest rates (1.5 percentage points). These differences compromised the profitability of the program: the *ex post* internal rate of return on program loans to connected households is 2 percentage points lower than the return on privately funded loans (on average 7%). These results are driven by differences for connected households, as there were no detectable differences for unconnected households.

Third, while committee members favored connected households and the program might not have directly reached unconnected households, the program indirectly benefited unconnected households by increasing the supply of overall credit available in the village. Aggregate borrowing increased by 24% in the sample villages within a year from the rollout of the program. Using high-frequency data, I exploit cross-village variation in the monthly rollout of the program to identify the short-term effects of the program on credit use for unconnected households. While connected households benefited directly from the program, unconnected households obtained loans from other lenders in the system. Event-study estimates for unconnected households show that borrowing from informal lenders increased by 30%; this result was mostly driven by loans from relatives. I also find suggestive evidence of an increase in formal borrowing for unconnected households, albeit at higher interest rates than those from the program. There was also re-lending: the probability of lending to other households increased by 2 percentage points in the case of connected households. Overall, spillovers mildly offset the difference in program borrowing between connected and unconnected households: back of the envelop calculations suggest that these effects only account for one-third of program-borrowing gap between connected and unconnected households.

This paper makes three main contributions to the literature studying community-based approaches to distributing public resources. First, it highlights the limitations of these approaches

groups among others. See Kaboski and Townsend (2005) for an in-depth assessment of these type of lenders.

to distribute productive resources when attributes of program beneficiaries are not easily observable by most community members. Unlike the context of poverty targeting, in the context of credit, the relevant targeting criteria may only be observable by direct economic interactions, strengthening the tension between information and favoritism. Alatas et al. (2012) provide evidence that households with connections to local elites are not more likely to receive cash transfers when resources are allocated by community members relative to a proxy-means-testing targeting criterion. The results from this paper show that this pattern may not hold in the case of credit and are consistent with evidence of favoritism in financial markets in the context of banks and firms (Khwaja and Mian, 2005; Haselmann et al., 2017). In addition, while Hussam et al. (2017) show that community members can identify productive households in India, this paper shows that accurate use of information may depend on social connections. In practice, both lack of information about unconnected households and favoritism can impose higher program-participation costs to households without the relevant connections, with consequences for poverty targeting, productive efficiency, and program sustainability. These losses should be considered whenever policy makers choose among alternative approaches to program delivery.

The second contribution to the targeting literature is methodological. The use of pre-program data has been central to the assessment of community-based approaches to allocate cash transfers to the needy (Alatas et al., 2012). However, studies evaluating the productive efficiency of community-based allocations rely on contemporary or post-program measures of productivity (Bardhan and Mookherjee, 2006b; Basurto et al., 2017). This paper improves previous empirical assessments by exploiting a long panel dataset to recover *pre-program* structural estimates of household productivity, which are unlikely to be affected by the program. In terms of results, using self-reported data collected after the implementation of a fertilizer subsidy program in Malawi, Basurto et al. (2017) provide evidence of a tradeoff between targeting the poor and targeting high-return households. Using post-program structural estimates of baseline household productivity, I show that such a tradeoff was not relevant in the more general case of credit.

Third, by studying a context in which active credit markets interact with the implementation of a large-scale program, this paper examines the targeting problem both from a partial and general equilibrium perspective. The literature has generally focused only on the targeting or screening process. This paper expands the analysis beyond the program and tests the consequences of the *de facto* targeting criterion on village credit markets. By providing novel evidence on the role of informal credit markets in attenuating targeting errors, this paper contributes to the literature documenting general equilibrium effects and spillovers from large-scale programs (Angelucci and De Giorgi, 2009; Muralidharan et al., 2017; Kaboski and Townsend, 2012). In particular, the results show that economic connections and political economy factors can affect not only the distribution of public resources in the village economy, but also the redistribution of these resources through markets (Kinnan and Townsend, 2012; Acemoglu, 2010). More broadly, the results suggest that a complete understanding of targeting problems should involve an analysis of how resources are redistributed across agents.

The results from this paper also build on the literature studying the introduction of micro-credit products in developing countries. A core concern in the development economics literature is that of delivering affordable credit to poor, high-productivity households to enable them to escape poverty traps (Banerjee and Duflo, 2010; Morduch, 1999). While the literature has mostly focused on studying the effects of the introduction of credit products on several household outcomes,[7] an empirical assessment of the productive efficiency of the allocation of credit in large-scale programs has not yet been provided. My results show that even with low intermediation and administrative costs, credit from the MBVF program did not reach poor, productive households. A comparison of these results with those from studies analyzing selection into credit highlights the importance of different screening mechanisms in credit markets. For instance, Beaman et al. (2014) show that high-return households select into credit in a context

[7]Banerjee et al. (2015) provide a review of six randomized controlled trials studying the introduction of microcredit products in a varied of contexts. In particular, Banerjee et al. (2015) and Crpon et al. (2015) document low uptake rates in contexts in which credit was not directly offered to entrepreneurs. Deininger (2013) analyzes the impacts of access to credit on members of self-help groups. Kaboski and Townsend (2012) also provide an assessment in the context of the MBVF program.

in which the screening mechanism is price.[8] This paper documents a less efficient result in a context in which the *de facto* screening mechanisms are social connections with local elites.

1.2 The village financial system and the Village Fund program

1.2.1 The village financial system

The context of this study corresponds to Thai villages, an environment in which most households own land (80%) and obtain over one-third of their revenues from agricultural activities (see Appendix table A.7). While most households obtain revenues from cultivation activities, the average household obtains revenues from 4 different economic activities: most households also obtain revenues from wage labor (78%), fishing and shrimping (40%) and off-farm family businesses (30%). To finance their economic activities, households borrow either from institutional lenders, informal lenders or relatives. Among institutional sources of credit there are formal lenders, mainly the state-owned Bank of Agriculture and Agricultural Cooperatives (BAAC), and quasi-formal lenders such as savings and credit groups and cooperatives.[9] In terms of the quantity of loans, half come from informal sources, while formal and quasi-formal sources of credit provide over 70% of the total loan amount in the village financial system.[10] On average, households hold more than one loan and around one-third of the households hold informal loans (see Appendix Table A.8), which have higher interest rates than formal or quasi-formal loans.

[8]They do so in the context of a micro-credit program in Mali, managed by an NGO with no government intervention at all.

[9]Quasi-formal institutions include organizations that have a set of procedures for recording their operations, but do not have a physical location. Examples of these are production credit groups (PCGs), women's groups and other village credit groups. See Kaboski and Townsend (2005) for a detailed description of these quasi-formal organizations in the Thai context.

[10]The top panel in Figure A.5 illustrates the structure of the portfolio of loans associated with the villages in the study sample, both in terms of the number of loans and the amount of credit provided before the program was implemented.

1.2.2 The Million Baht Village Fund program

The Million Baht Village Fund (MBVF) program consisted of an initial transfer of THB 1 million (USD 22,500 in 1999 values), from the Government of Thailand to rural and peri-urban villages.[11] The aim of the program was to stimulate the village economies by expanding access to credit; program funds were used as seed capital for the creation of revolving credit funds in 95% of all villages in Thailand. [12] Moreover, the program increased the aggregate gross lending portfolio by 24% during the first year of its implementation in the sample villages, and modified significantly the composition of the portfolio of loans in each village (See and Appendix Figure A.5). The program offered loans at an average interest rate of 7% per year, which was the lowest rate in the market at that time: The average interest rate for other institutional loans was 11% per year . The program represents an unexpected event in that it was announced following a change in government and rapidly reached borrowers: As of the second year of implementation, the program had provided individual liability loans to 62% of households in the study sample.

The MBVF program differs from formal lenders in its management, relying on community members to manage credit funds. While there are other local savings and credit groups in which community members manage funds, they differ from the MBVF program in the way that they are funded: The MBVF is mostly subsidized, and local credit and saving groups are self-funded.[13] In each village, the MBVF program is managed by a village fund committee (VFC),a group of 10-12 elected community members that is responsible for evaluating loan applications and monitoring loans.[14] Committee members generally met once or twice a year to review loan applications. While the program was governed by a set of regulatory guidelines, committee

[11] Average loan size is approximately USD 450 which represents roughly 25% of a households's yearly income.

[12] A detailed discussion of the application process that villages were required to follow to get access to the funds and the way in which those funds were delivered is provided by Kaboski and Townsend (2012), Boonperm et al. (2013), Menkhoff and Rungruxsirivorn (2011) and Haughton et al. (2014).I do not address that process here as all of the villages in the sample participated in the program.

[13] In order to borrow, households were require to purchase a share of the fund, at a very low costs. However, the funds themselves come from a one-time transfer by the Government.

[14] The members of the Village Fund Committee were elected for a 2 year term in a transparent setting and received a small compensation for their services (Menkhoff and Rungruxsirivorn, 2011), however Haughton et al. (2014) documents that most of the members continued in the position for several years.

members had full discretion to approve or deny applications and set loan amounts, terms, and the initial interest rate.[15] Although the Central Government provided villages with incentives for sustainable management and sanctions in case of mismanagement, there were no direct incentives for committee members.

1.2.3 Local elites and the MBVF program

Each Thai village is governed by a village head and a group of advisors who make up the village council; they are hereinafter referred to as the "local elite". The Village Council members are elected by villagers, appointed by district authorities, and usually serve in office until retirement.[16] The Village Council represents the main link between community members and higher-level authorities. For instance, village council members attend district meetings, collect resources from villagers for religious celebrations or public works, and oversee resolution of disputes between villagers (Moerman, 1969; Mabry, 1979). In the study sample, Village Council members are richer, have larger extensions of land, and are more likely to have off-farm family businesses (see Appendix Table A.9).

The village fund committee was *de jure* an independent entity, but it is possible that the local elite, had enough *de facto* authority to influence committee decisions. Although the election of village fund committee members is intended to induce accountability in the allocation of loans, committee members may have incentives to favor their political supporters or households with connections to the local elite. For instance, when elections could not take place, the committee members were appointed by the village Head.[17] The local elites could indirectly influence committee members through their economic or family connections: On average, 46% of households in the sample report transacting with village council members during the two years

[15] The most important of these regulations were that loans could not exceed THB 20,000, a positive interest rate had to be imposed on all loans, the initial loan term could not exceed one year, and collateral could not be required, although households had to have one or two cosigners.

[16] This was the case during the study period. However, a reform in 2011 established 5 year terms, but allowed Village Heads to run for reelection.

[17] Haughton et al. (2014) document that 15% of village fund committee members were appointed directly by either the Village Head or the Village Council

preceding the program and 13% of sample households are direct relatives of elite members (see Appendix Table A.10). In addition, relatives of the local elite could end up in charge of the funds even in transparent elections. [18] Moreover, households with business connections to local elites could use their privileged position to influence loan allocation decisions or to obtain preferential treatment. In such a context, the potential gains in information from decentralizing the allocation of resources to community members could be undermined by rent-seeking behavior (Bardhan and Mookherjee, 2005).

1.3 The Village Fund committee as a social planner

The central aim of this paper is to evaluate the allocation of resources by community members. The program's stated objective was to establish credit funds in order to expand access to institutional credit and promote career development and income generation (Government of Thailand, 2004), which suggest that poverty, productivity and repayment were important dimensions to be considered. For instance, access to institutional credit was particular low among the poor[19], the government claimed publicly that resources were allocated to productive activities (Phongpaichit and Baker, 2004), and the sustainability of the village funds relies heavily on repayment. However, there were no explicit guidelines regarding the target population. Thus, theoretical analysis of the optimal targeting rules will provide insights to understand the different sources that affect the allocation of credit by community members.

In this section, I sketch a simple theoretical framework characterizing the optimal allocation of public resources and apply this framework to the context of the MBVF program. Drawing on the notion that the village fund committee allocates loans in order to maximize a village welfare function as if the committee was a benevolent social planner, the theoretical framework sketched in this section expands the work of Bardhan and Mookherjee (2006b) by

[18](Cruz et al., 2017) document that individuals who belong to more central families are more likely to be elected for office in the Philippines

[19]Per-capita consumption was 16% lower for households without access to institutional credit at baseline (See Panel A from Appendix Table A.12.

allowing villagers to differ in terms of repayment. The insights from the theoretical framework imply that evaluating the allocation of credit involves considering whether the resources were provided to poor, high-productivity households.

The general problem of community members in charge of allocating public resources is represented in (1.1). Community members choose the allocation of resources $\mathbf{b} = \{b_i^*\}_{i=1}^{i=N_v}$ that maximizes the weighted sum of utilities corresponding to their fellow villagers (N_v) subject to feasibility, sustainability and other constraints imposed by the central government ($F(\mathbf{b})$):

$$\max_{\{b_1,\ldots,b_{N_v}\}} \sum_{i=1}^{i=N_v} \psi_i V(b_i)$$

$$\text{s.t.}$$

$$F(\mathbf{b}) \leq 0 \tag{1.1}$$

Political favoritism, social norms, and preferences may determine the weights associated to each village member (ψ_i), which I assume are exogenous to the allocation problem. V_i denotes a household i indirect utility function which is increasing and concave in b_i–i.e., the value function from the corresponding household optimization problem–. Consider the problem of MBVF committee. For the sake of simplicity, suppose that households repay their loans with an exogenous probability q_i which is known to the committee, and that loans are provided at a government-imposed interest rate r. In this case, community members solve the problem in (1.1) facing a sustainability constraint of the form: $F(\mathbf{b}) = \sum_{i=1}^{i=N_v} b_i - \sum_{i=1}^{i=N_v} q_i(1+r)b_i$. The first order conditions imply:[20]

[20]More generally, the optimal allocation of resources implies that the ratios between the marginal weighted utility of obtaining public resources and the marginal costs of satisfying allocation constraints are equal across all villagers.

$$\frac{\psi_i \frac{\partial V_i}{\partial b_i}}{\frac{\partial F}{\partial b_i}} = \frac{\psi_j \frac{\partial V_j}{\partial b_j}}{\frac{\partial F}{\partial b_j}}$$

$$\forall i, j$$

$$\hat{\psi}_i \frac{\partial V_i}{\partial b_i} = \hat{\psi}_j \frac{\partial V_j}{\partial b_j} \tag{1.2}$$

$$\hat{\psi}_i = \frac{\psi_i}{1 - q_i(1+r)}$$

$$\forall i, j \tag{1.3}$$

where $\tilde{\psi}_i$ denotes the effective weight after incorporating the potential loss from providing a loan to a given household (i). In words, MBVF committee members will allocate resources such that the weighted marginal utilities from receiving extra-liquidity are equal across all villagers. Note that while committee members will punish households with a low probability of repayment, they may still deliver credit to risky households if their personal weights ψ_i are high enough for a particular households–i.e., a relative–. If the marginal utility of an extra unit of liquidity $\frac{\partial V_i}{\partial b_i}$ is diminishing with respect to b_i, then equation (1.2) implies that, conditional on the effective weights, it is optimal for MBVF committee members to provide resources to households who would benefit the most out of the program–i.e., high $\frac{\partial V_i}{\partial b_i}$–.

The identity of these households depends on the economic context in which they make their optimal decisions regarding consumption and input use. For instance, in a context of complete markets, optimal input choice should not depend on household characteristics (i.e., wealth) as households behave as unconstrained profit maximizer firms. In that context well functioning credit markets will deliver resources to all profitable projects, and the marginal utility from a program loan should not be a function of poverty. However, in contexts of incomplete credit markets, input use will be a function of household's characteristics, and the marginal utility of a household from obtaining a loan from the program will depend on the type of frictions that characterize rural credit markets.

For ease of exposition I discuss two frictions in credit markets: borrowing constraints and high borrowing interest rates which would make self-financing a more attractive option for households even in absence of borrowing limits.[21] In the case of borrowing constraints, a loan from the program will relax these constraints by providing access to more liquidity. In the second case, because the program offered credit at the lowest interest rate in the village, obtaining a loan for the program would lead to a reduction of the interest rate at which unconstrained households borrow. The following two propositions characterize the household marginal utility derived from a program loan in both cases.

Proposition 1: *If households face borrowing constraints, the marginal utility of relaxing this constraint is decreasing in initial wealth. Moreover, the marginal utility of relaxing a household's liquidity constraint is an increasing function of household productivity if the distortion in the optimal choice of inputs is large. Proof:* See Appendix section A.4.

Intuitively, as richer households can substitute credit with initial wealth, conditional on productivity, their optimal choice of inputs will be less likely to be distorted by the presence of liquidity constraints and the shadow price of relaxing such a constraint will be smaller; this substitution may not be possible for poor households. In the case of productivity, as liquidity-constrained households cannot obtain funds to finance their optimal inputs choice, the marginal product of inputs will exceed the costs of financing inputs. This distortion will be higher for high-productivity households. As poor, high-productivity households are more likely to face binding liquidity constraints and experience higher distortions in their optimal choice of inputs, their marginal utility from a program loan will be higher.

Proposition 2:*If households do not face binding borrowing constraints but face high borrowing interest rates, the marginal utility from a reduction in the interest rate is a decreasing function of initial wealth and an increasing function of household productivity Proof:* See Appendix section A.4.

Intuitively, conditional on productivity, households with low initial wealth will borrow more and would benefit from a decrease in the interest rate. In contrast, as optimal input choice is

[21] Several models could generate such a friction. For instance, the existence of intermediation costs or information rents would create a gap between the interest rates obtained by deposits and the borrowing interest rates, making self-financing a cheaper option than borrowing.

increasing in household productivity, conditional on initial wealth, more productive households will demand more inputs, will borrow more and hence will benefit the most out of a decrease in the interest rate.

Propositions 1 and 2 and the first order conditions from the VF committee's problem (1.2) imply that if the probability of repayment is constant across households, and committee members weight all households equally, it is optimal to deliver more resources to poor, high-productivity households. In practice, any deviations from such behavior should be explained either by differences in repayment probabilities q_i, differences in committee member's preferences for a particular household ψ_i or the inclusion of further restrictions to the committee member's problem. In the case of the MBVF program, targeting non-poor, low-productivity households would be justified if these households had high repayment probability. However, if this was not the case, then targeting non-poor, low-productivity households should be explained by committee members preferences weighting other household characteristics unrelated to poverty, productivity or repayment such as political connections or differences in the cost of obtaining information.

Motivated by the implications of the previous theoretical framework, this paper reports results from three empirical exercises analyzing the allocation of loans from the program: First, I test whether village committee members delivered credit to poor, high-productivity households. Second, I compare the relative performance of the actual allocation in terms of poverty targeting and productive efficiency with benchmark counterfactual allocation criteria: means testing and repayment score. The former will test the empirical relevance of a trade-off between poverty and productivity, while the latter will test the extent to which there is a trade-ff between targeting high-repayment probability and high-productivity households. Third, I analyze the extent to which socioeconomic connections with local leaders relate to deviations from the optimal target population, and the extent to which these deviations are explained by information or favoritism.

1.4 Data and measurement

This study uses data from 172 waves of the Townsend-Thai Monthly Survey (Townsend, 2014). Starting in September 1998, the survey covers two years prior to and 12 years after the program's implementation. The survey follows a sample of 709 households from randomly selected villages corresponding to four provinces in Central and Northeast Thailand.[22] The dataset provides detailed information regarding transactions among households, the portfolio of loans held by each household, input use, and household financial statements.

While Kaboski and Townsend (2012) and Kaboski and Townsend (2011) used the Annual Townsend-Thai dataset to exploit cross-village variation in order to study the effects of the program on household outcomes, the monthly version of the survey is optimal to analyze how resources were distributed within a village. The monthly panel provides detailed information regarding socioeconomic interactions and loan repayment which is not available in the yearly survey. While the annual survey covers a high number of villages, it includes a small number of households in each village. In contrast, the monthly survey includes on average 44 households per village which allows for within-village analysis.

Out of 709 households who were interviewed in the first wave of the survey, 509 households were interviewed in the subsequent 171 waves, and, on average, 670 households are interviewed in each wave. As most of the analysis of this paper concerns comparisons of pre-program characteristics corresponding to the first 40 waves of the survey, I focus on the unbalanced panel of 671 households for whom data regarding baseline interactions were available and present robustness checks using the balanced sample for results that are obtained using variation over time (see Appendix Section A.3.1).

[22] Provinces: Chachoengsao, Lop Buri, Buri Ram, and Si Sa Ket.

1.4.1 Measuring poverty

I approximate poverty using the average baseline per-capita consumption corresponding to the year preceding the program. I focus on per-capita consumption rather than wealth to capture the short-term component of poverty.

1.4.2 Measuring pre-program productivity

To assess productive efficiency, I focus on household total factor productivity as the main variable of interest. I exploit a panel data set to estimate the parameters from a production function which I use to recover pre-program estimates of household total factor productivity. I estimate a production function corresponding to household aggregate value-added by implementing the two-stage approach proposed by Olley and Pakes (1996); Levinsohn and Petrin (2003) and Ackerberg et al. (2015), using intermediate inputs as the proxy variable. I approximate output using total revenues from all household economic activities which include agriculture, livestock farming, fishing and shrimping, off-farm family businesses and wage work outside the household. Capital is measured as the value of the stock of household fixed assets which include land, value of livestock, real-state, appliances and agricultural equipment. Labor is measured as total hours per year of labor provided by household members (on average 85% of total labor) and workers outside the household. Intermediate inputs are measured as the value of inputs purchased outside the household which were used in revenue-generating activities.[23] I also provide robustness checks using productivity estimates from a gross-revenue function estimated by GMM following a dynamic-panel approach.

The choice of the empirical approach implies a series of assumptions which are discussed in the following paragraphs. First, because there is heterogeneity in the sources of income in the households in the data and because most households have several sources of income,[24] I

[23]These inputs include fertilizer, seeds, hired labor from other households, feed for cattle, and other tools required for non-farm family businesses.

[24]A behavior typical of rural environments in which household manage risk by diversifying their sources of income (Alderman and Paxson, 1994). Panel C from Appendix Table A.7 shows that on average a household obtains

aggregate revenues and input use all household's economic activities. This decision comes at a cost of interpretation of the elasticities, since a production function is specific to one particular process. [25] As the goal of this paper is not to compare elasticities across sectors but to quantify variations in output conditional on input use, the analysis in this paper focuses on productivity measures from all household activities.

Second, as there is heterogeneity in household economic activities and in the intermediate inputs contributing to the generation of revenues, I estimate a value-added production function. [26] However, a value-added approach assumes that households can't produce any output without intermediate inputs–i.e., the underlying production function is Leontief on intermediate inputs (Ackerberg et al., 2015; Gandhi et al., 2016)–; which is a strong assumption in the context of subsistence agriculture but a weak assumption when households have several sources of income such as off-farm business.[27]

Third, I choose a choice-based approach (Ackerberg et al., 2015) to recover productivity estimates over a dynamic-panel approach (i.e., Anderson and Hsiao (1982)). While both rely on assumptions regarding the timing of capital and labor choices, they differ in the assumptions regarding the dynamics of unobserved productivity and the way in which households accommodate productivity shocks. The former does not impose a functional form in the dynamics of unobserved productivity but the latter imposes linearity (productivity follows a first-order autoregressive process). However, the former uses intermediate inputs to proxy for changes in

revenues from 4 different sources: typically cultivation, labor provision, livestock and off-farm family businesses.

[25]This problem is typically assessed in firm-level analysis by estimating production functions by industries. However the concept of "industry" is not applicable in the context in which households have several sources of income and sort in and out a particular type of business. For instance, Nyshadham (2014) documents that households transition in and out of off-farm businesses fairly often in the Thai villages of this sample. In the data, all households have at least two sources of revenues.

[26]There are other reasons for the choice of a value-added function approach as opposed to a revenue function. The first reason is to minimize the chances of double accounting in cases in which a households uses the output of one activity as intermediate input for another–i.e, using agricultural output as feed for livestock–. The second reason follows from the discussions on Ackerberg et al. (2015), and more generally in Gandhi et al. (2016), regarding the lack of identification of the elasticities corresponding to intermediate inputs in gross revenue functions in choice-based methods such as the one used in this paper.

[27]In a nutshell, this assumption means a household can't produce crops without fertilizer, which may not be true. However, adoption of fertilizer and seeds is quite high in the data. This assumption is also weak when we think of households having several sources of income.

unobserved productivity under the assumption that households can freely adjust intermediate inputs. This assumption will be violated if there are adjustment frictions. In the context of the sample villages, while there might be borrowing constraints, households hold large amounts of inventories which may allow them to adjust intermediate inputs to productivity shocks.[28] More formally, Section 1.4.2 provides results from a graphical test for this assumption proposed by Levinsohn and Petrin (2003), and from a test for rigidities in input adjustment suggested by Shenoy (2017).

Identification of the production function

In this section I describe the main behavioral assumptions needed to identify a value-added production function, and defer a detailed discussion of these assumptions, estimation details and specification checks to Appendix sections A.2 and A.2.1.[29] Formally, the goal is to recover pre-program estimates for ω_{it}: productivity shocks, observed by the households but unobserved by the researcher. Let y_{it} denote total value added in logs [30], k_{it} denote log capital, l_{it} denote log labor, and ε_{it} denote unforeseen exogenous shocks to production. The log value-added production function is:

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_k k_{it} + \omega_{it} + \varepsilon_{it} \tag{1.4}$$

The empirical challenge is to consistently estimate the parameters from equation (1.4) in a context in which households choose labor and capital in response to productivity shocks (ω). Levinsohn and Petrin (2003) and Ackerberg et al. (2015) provide a solution by using variation

[28]See Samphantharak and Townsend (2010) for a detailed description of household financial choices in context of incomplete credit and insurance markets in these villages. In fact, ongoing work by Kinnan et al. (2017) find that less central households in the village socioeconomic network have higher levels of inventory to accommodate production in contexts of idiosyncratic shocks.

[29]Appendix section A.2 describes the theoretical model consistent with the empirical estimates, the moment conditions required for estimation, and describes the estimation procedure. Appendix Section A.2.1 provides a test for over-identifying restrictions and discusses other alternative specifications.

[30]Value added is computed by subtracting the value of purchased inputs from the gross revenues generated by a household in a given time period.

from a proxy variable (m_{it}) that monotonically responds to productivity shocks to control for variation in productivity, conditional on labor and capital choices.[31] I use the value of inputs to proxy for variation in productivity. Hence, the main identification assumption is that households flexibly adjust their demand for intermediate inputs in order to accommodate productivity shocks in a strict monotonic way ($m_{it} = f_t(\omega_{it}; l_{it}, k_{it})$), , conditional on capital and labor choices. Strict monotonicity allows me to model variation in productivity shocks as a function of intermediate inputs ($\omega_{it} = f_t^{-1}(m_{it}; l_{it}, k_{it})$) and use this function to control for variation in productivity.[32]

Four other assumptions are necessary to recover total factor productivity estimates. First, I assume that, conditional on village-specific shocks, ω_{it} follows a first-order Markov process. Second, I assume that the stock of capital is a predetermined with respect to productivity shocks–i.e., it is a function only of investment and the stock of capital in the previous period ($k_t = k(i_{it-1}, k_{it-1})$)–. This is operationalized by measuring capital as the stock of fixed assets at the beginning of each calender year. Third, I allow labor choices to be flexibly adjusted in response to contemporary productivity shocks, but assume that labor decisions are not correlated with future shocks to productivity. Finally, as physical measures of output and intermediate inputs are not available, I include village-year fixed effects, and assume that input and output prices are common for households in the same village in a given year. [33]

Following the estimation process detailed in Appendix Section A.2.1. Appendix table A.13 presents estimates for the elasticities of labor and capital corresponding to equation (1.4). Column (3) presents results for my preferred specification which uses 13 years of panel data to compute production function elasticities which are then used to compute pre-program households productivity and provides evidence of constant returns to scale. Column (4) reports elasticities obtained by instrumenting pre-determined capital with its first lag to account for potential

[31] Most firm-level studies either use investment as the proxy variable (Olley and Pakes, 1996), or intermediate inputs, such as electricity, as proxy variables (Levinsohn and Petrin, 2003).

[32] This motivates the first stage of the estimation approach. However, as discussed by Ackerberg et al. (2015) and Gandhi et al. (2016), none of the elasticity estimates are identified from equation (1.4). See Appendix section A.2.1 for a discussion of the moment conditions required for the identification and estimation of the elasticities β_l,β_k.

[33] Accounting for the influence of prices requires incorporating a demand-system to the estimation framework and exploit variation in aggregate demand which is not available in this context (De Loecker, 2011).

measurement error. The results are robust to using only data corresponding to pre-program periods (1999-2001) and a balanced panel of non-attriter households for the estimation (Columns (5) and (6)).[34] Finally, using an overidentified version of the model (see Column(7)), I find that it is not possible to reject the null that the model's structural restrictions hold.[35] Finally, Appendix table A.14 shows that results are fairly robust to alternative measurements of capital, labor, and revenues and to estimating different production functions for households whose primary source of revenues are related to agriculture (see Panel B).

Validation and discussion of the main identifying assumption

The main assumption of this approach is that, conditional on capital and labor, there is a strict monotonic relation between intermediate inputs and productivity. Appendix Figure A.6 provides a graphical examination of this assumption by plotting the productivity estimates as a function of the value of purchased inputs, after partialling out the variation from capital, labor, and village-year shocks (Levinsohn and Petrin, 2003). I find evidence of a strict monotonic relation between productivity and the proxy variable. Table A.15 reports results from the test for adjustment rigidities suggested by Shenoy (2017) and shows that there is no evidence of rigidities in the adjustment of intermediate inputs. [36] An alternative way of relaxing this assumption is to estimate a value-added function using a dynamic-panel approach (Anderson and Hsiao, 1982) through GMM after "ρ-differencing" equation (1.4). Columns(8) and (9) from Appendix table A.13 reports elasticities from this approach which are similar to the benchmark estimations obtained following a choice-based model. Columns(10)-(11) relax the value-added assumption

[34]Production function elasticities using only pre-program data are very similar, almost identical. I base my conclusions on pre-program productivity measures using elasticities corresponding to 13 years which are more conservative than results using only pre-program data.

[35]Note that although an overidentified system would deliver more precisely estimated coefficients, the fact that I only observe two years of baseline data limits the estimation of TFP precisely for the baseline years, which are the main input for the analysis in this study. More importantly, consistency of these estimates depends on the correct specification of the variance-covariance matrix.

[36]To implement the test, I first regress value added on a flexible third-order polynomial of current choices of capital, labor and intermediate goods and compute the residuals. Second, I test whether flexible polynomials of lags for capital, labor and intermediate inputs have explanatory power on the residuals from the first regression. Rejecting the null of no explanatory power of lagged inputs will be supportive of rigidities in the market for inputs.

and report factor elasticities from a gross-revenue function estimated through GMM following a dynamic approach. Identification in this case comes at the cost of assuming that there are rigidities in the adjustment of intermediate inputs which allow the econometrician to use input choices in previous periods as instruments for current inputs (see Appendix section A.2.1 for details). As no approach is perfect, I report results from estimates of total factor productivity following the dynamic panel approach for all the comparisons in this paper. I also report results using direct measures of financial profitability following Samphantharak and Townsend (2010), such as the asset-turnover ratio and profitability margins per unit of revenue.

1.4.3 Measuring repayment behavior

I track the full stream of disbursements and payments associated to each loan reported in the survey, until a loan is fully paid or defaulted on, and use these data to construct four indicators of loan performance: First, I count the number of times a borrower failed to make a payment and construct delinquency rates for each loan. Second, I compute an indicator of whether the loan experienced any delinquent payment. Third, I identify whether a loan was repaid in a longer period than its original term. Fourth, I measure returns to the lender using the *ex post* internal rate of return on each loan in order to have a common measure of loan profitability that accounts for loan size and changes in the loan payment schedule. Although default is observed, there is little variation on this as default rates are mostly zero in the data.

I complement these indicators with information regarding the loans' initial characteristics such as size, term, the need for collateral, or a cosigner. Initial interest rates were self-reported and are converted to yearly values by multiplying them by 12 or 52, in the case of monthly and weekly rates, respectively. To recover baseline delinquency rates for each household and avoid sample selection, I take the average over all the loans that were obtained before the program, including loans from informal lenders.[37]

[37] Use of institutional credit was not universal and would limit the ability to use pre-program information for households without access to institutional credit.

1.4.4 Measuring connections with local elites

The dataset contains information regarding different types of socioeconomic interactions between households in the village.[38] To prevent potential effects of the program on network formation, I use only pre-program interactions to identify connections. With the aim of capturing several dimensions of social interactions, I use information on all types of transactions among community members.[39] Thus, a household is defined as connected with the local political elites if any of its members reports either being a member of the village council, or a first-degree kin of a council member, or having engaged in at least one interaction, of any type, with any village council member during the baseline periods.[40]

There are two limitations to these connections measures: First, by using the extensive margin of transactions to define connections, it is possible that a household is identified as connected because of one isolated interaction. Since the relative salience of each interaction cannot be identified nor valued, when pertinent I provide robustness checks using an alternative definition of connectedness based on Principal Component analysis across the different types of transactions. Second, since only village council members in the sample can be identified, as opposed to all village council members, there is a potential downward bias in measuring connections with elites. Thus, the results based on comparisons between connected and unconnected households represent lower bounds of the true differences. However, this bias should not be strong as village council members represent only 10% of the households and at least one committee member is observed in each village in the sample.[41]

[38] The transactions can be roughly categorized in seven groups: output sales or purchases, asset purchases or relinquishments, transfers (gifts), borrowing (lending), paid labor provision (demand), unpaid labor exchange, and other inputs, which include materials purchases (sales) as well as advising and mentorship.

[39] Summary statistics by interaction type are provided in Appendix Table A.11.

[40] While other measures–such as geodesic distance (shortest path)–might provide a better approximation of the distance between a household (node) and the elites in the network, these measures are subject to potentially high biases arising from the sampled nature of the transaction data. As noted by Chandrasekhar and Lewis (2017) there is non-classical measurement error when connections are computed using only a sample of the nodes in a network and the associated bias gets more complicated to tackle when network statistics that involve indirect connections are employed (e.g., the path length to the closest elite member).

[41] Appendix Table A.9 shows demographic characteristics by type of connection with the elites. Appendix Table A.10, complements this information by presenting summary statistics of baseline connections with local elites.

1.5 Targeting analysis

In this section I analyze first if the program was successful at reaching poor,productive households, and then I test: *i)* whether there was a tension between targeting the poor and delivering credit to high-productivity households and *ii)* whether allocating resources based on a repayment-score would have led to a more equitable or productively efficient allocation.

While the program currently operates in several villages, I focus on the first two years of the program for two reasons.[42] First, I compare baseline characteristics between program beneficiaries and non-beneficiaries, and to the extent that consumption and productivity responded to the program or significantly varied over time, baseline characteristics are more representative of the context around the rollout of the program. Second, modifications were made to the program years after its rollout, such as changes in the orientation of the funds to community improvement projects, sanctions for poorly managed funds, and rewards for successful ones.

1.5.1 Comparisons of program beneficiaries and non-beneficiaries

I find that the program did not target resources neither following a poverty targeting nor a productive efficiency criterion. Figure 1.1 depicts the cumulative distribution function of per-capita consumption and value-added total factor productivity for program beneficiaries and non-beneficiaries. Loans from the program were allocated to richer households; the distribution of per-capita consumption for program beneficiaries first-order stochastically dominates that of non-beneficiaries. Regarding productivity, the program on average targeted households from the middle of the distribution of total factor productivity and was less likely to target high-productivity households: less than half of high-productivity households (i.e., top 25% of the distribution of productivity) obtained loans from the program.

Table 1.1 quantifies the extent to which the program misdirected resources in relation to both the poverty targeting and the productive efficiency. Panel A shows that on average, the

[42] I choose two years in order to capture households that may not have needed credit during the first year but obtained credit during the second year.

program targeted wealthier households and the differences arise at the bottom of the distribution of per-capita consumption; the 10th percentile is 22% higher for households who had access to credit from the program. In terms of productivity, the 75th percentile of the distribution of total factor productivity is 15% lower for program beneficiaries than for non-beneficiaries. This pattern is similar in the case of complementary measures of productivity and is particularly stronger in the case of the alternative gross-revenue productivity estimates obtained by the alternative dynamic panel approach (see bottom panel).

1.5.2 Poverty targeting , productive efficiency and repayment

Basurto et al. (2017) highlight the importance of distinguishing between poverty targeting and poverty reduction, which may arise in a context in which the poor may not necessarily be the most productive. To test the salience of this tradeoff, I evaluate the the allocation achieved by community members in relation to the allocation that would have been observed had loans been offered according to a pro-poor criterion–i.e., means testing (MT). This criterion aims to capture the allocation that would have been observed if the Village Fund committee placed a high weight on delivering resources to the poor.

Similarly, it could be the case that committee members faced a tradeoff between targeting poor, high-productivity households and households with a high expected repayment rate. To test the importance of this tradeoff, I compare the the allocation achieved by community members to the allocation that would have been observed had loans been offered according to a repayment score based on predicted baseline probability of missing a due payment for institutional loans. While an allocation based only on a scoring model may not fully reflect the choices that would be made by a traditional MFI credit officer, it is still policy relevant as it captures information regarding *ex ante* risk which might be costly to the lender (Schreiner, 2000) and is informative regarding the decisions that would have been made by a risk-averse lender.

In order to identify households who would have been targeted by a means-testing criterion, I compute the average stock of per-capita gross assets over pre-program periods and construct within-community wealth rankings.[43] Using these rankings, the households with the lowest positions are classified as the MT target group and are selected into this group until reaching the uptake rates of the MBVF during the initial two years of the program (avg. 62%). I follow a similar approach using percentile rankings of predicted delinquency rate giving priority to households with low predicted delinquency rate.[44] This process classifies households into four groups: households that would have been targeted by both the program and the respective alternative criterion, households that would have been excluded from both allocations, households that were reached by the MBVF program but would have been excluded by the alternative criterion, and households that were excluded from the VF program but would have been targeted by the alternative criterion (see Appendix Table A.1).

Means testing would have targeted a different set of people: over 40% of households targeted by the program would have been excluded by the MT criterion. While these households are by construction richer, they are on average more likely to be low-productivity households (bottom 25% of the productivity distribution). Table 1.2 compares means and quantiles of per-capita consumption and productivity between households who would have been targeted by the program but would not have been eligible under the MT criterion and households who would have been targeted by the MT criterion but were not program beneficiaries.[45] Overall, the results show that MT outperforms the program under all metrics. Contrary to the program, a means-testing criterion would have offered credit to the ultra poor and simultaneously would have

[43]Gross assets data is obtained from the households' balance sheets compiled by Samphantharak and Townsend (2010). Gross assets include non-land fixed assets (i.e., household assets, cultivation and family business assets), livestock and land value.

[44]To recover baseline credit scores related to loans from institutional lenders, I use a subset of households with pre-program access to institutional credit (i.e., credit from formal or quasi-formal lenders) to estimate a model of baseline delinquency rate for institutional loans as a function of household demographic and productive characteristics. I then use the coefficients of that model to generate predicted delinquency rates for all households in the sample and construct percentile credit-score rankings in each village assigning a higher credit score to households with low predicted delinquency. The household characteristics include household head age, gender and years of education, total land holdings and shares of total revenues by source. All continuous variables are grouped by quartiles and are interacted with household head gender in the model. The model also includes village fixed effects and overall explains over one-quarter of the variation in the probability of exhibiting delinquent payments in the baseline period.

[45]See Appendix Figure A.1 for an illustration. A characterization of targeted households is presented in Table 1.4 (Columns (6)-(7)).

excluded households belonging to the bottom 25% of the distribution of total factor productivity.

Over a third of households who obtained credit from the program would have been excluded by a repayment-score targeting criterion. These households were more likely to be low productivity households (bottom 25%) though also less likely to belong to the top 25% of the distribution of per-capita income. Relative to the program, a repayment-score criterion would have offered credit to a higher share of poor households, a lower share of households in the middle of the per-capita consumption distribution and a higher share of households from the top of the distribution. On average, reallocating resources to high-repayment probability would not be related to a cost in terms of equity with respect to the program. In terms of productivity, targeting credit following a repayment-score criterion would have delivered credit to the households with the highest productivity. This differences in terms of efficiency are sizeable: households who obtained credit from the program but would have been ineligible by a repayment-score criterion were on average 12% less productive than households who did not obtain credit from the program but would have been offered credit by the alternative criterion. The results are driven by differences in the top of the productivity distribution (see Table 1.3). Again, the same pattern is observed across different proxies for productivity. Overall, reallocating resources from program borrowers with low repayment probability to high-repayment probability households who did not obtain program credit would yield an average increase in productivity of 4.5%.[46]

1.5.3 Discussion

I find that resources from the program were not optimally allocated neither with respect to poverty targeting, nor with respect to productive efficiency. Moreover, the allocation of resources is not consistent with an allocation that would have targeted households with the highest repayment probability measured by a scoring model. Thus, the allocation achieved by

[46] This result is obtained by simply dividing the productivity gap from reallocating resources (12%) and scaling it down by the share of program borrowers who would be ineligible under the repayment criterion (0.38).

the elected Village Fund committee is unlikely to have been motivated by concerns regarding equity, productive efficiency or risk. These results contrast sharply with experimental evidence from a NGO-led credit program in Mali in which low-return households self-selected out from credit (Beaman et al., 2014). The main explanation is the screening mechanism used by each program. The program in Mali had zero government intervention allowing price to be the main relevant screening mechanism. Section 1.6 show how the relevant screening mechanism in the Thai case was related to political connections.

In this paper, I study a government-funded program managed by elected community members who have full discretion in the application process and in defining loan conditions. The theoretical framework discussed in Section 1.3 suggest that failure to provide credit to poor, high-productivity households might be related to Village Fund committee members weighting their fellow villagers based on different criteria. A compelling hypothesis is that committee members weighted more households with socioeconomic connections to local leaders. However, there are other factors that could influence the way in which Village Fund committee members weight each household such as externalities of financing a particular project or simply lack of demand for credit; Section 1.6 directly examines the role of connections with local authorities in the allocation of resources and discuses alternative compelling explanations while Section 1.7 discusses concerns regarding the demand from credit for households with lower chances of obtaining credit exploiting variation in the supply of credit in the village financial system induced by the program's rollout.

1.6 Access to credit from the program, connections with local elites, and favoritism

A central concern related to efforts to decentralize the allocation and management of public resources to community members relates to perverse incentives that may lead to favoritism or resource capture. However, the appeal of decentralized approaches to policy members relies on the idea that social connections may transmit information regarding program beneficiaries which might be costly to obtain by traditional policy makers. In this section I first show that households with connections to local elites are more likely to obtain credit from the program. Second, I discuss the extent to which this relation is related to information and/or favoritism.

Figure 1.2 depicts raw averages of the probability of obtaining a loan from the program for elite members, connected households and disconnected households. Resources from the program were disproportionately allocated to households with connections with the elites. This pattern is not explained by differences in baseline repayment history (see Appendix table A.2). In terms of poverty targeting or productive efficiency, connected households were neither poorer nor more productive. Panel A from Appendix table A.3 shows that while on average connected households are similar to unconnected households in terms of per-capita consumption, among the poorest households, connected households are better off: The 10th percentile in the distribution of per-capita consumption is 12% larger for connected households. Panel B shows that connected households were on average as productive as unconnected households; however the 75th percentile of total factor productivity is 17% lower for connected households. This pattern is even stronger across other measures of productivity (see panels C-E), and is precisely observed in the regions of the per-capita consumption and productivity distributions where program beneficiaries differed from non-beneficiaries (see table 1.1).

To understand the extent to which village fund committee members use connections to proxy for desirable borrower characteristics, Table 1.4 shows regressions of the probability of obtaining a loan from the program during the first two years of its implementation on connections with the elites controlling for the number of links each household has in the socioeconomic network (degree), a set of baseline demographic characteristics, productive characteristics, credit history, and village fixed effects.[47] Column (1) shows that connected households are 18 percent-

[47]The baseline delinquency rate is computed as the number of times a household fails to make a loan payment as a share of all payments due for loans obtained before the introduction of the program. Although only 60% of households ever reported holding a loan from formal or quasi-formal sources in the baseline periods, most households reported holding loans from either informal lenders. I use information regarding the history of payments of each reported loan, regardless the source, to compute delinquency rates and avoid dropping observations from

age points more likely to obtain credit from the program and that these correlation is reduced to 10 percentage points after controlling for relevant household characteristics (see Column (3)), baseline access to credit (Column (4)) and productivity (Column (5)). Column (6) decomposes connections with local leaders by type of connection–i.e., council membership, connection through transactions, or being a first-degree relative–and shows that the correlation is driven by council membership and direct transactions with council members. These results suggests that connections carry important information and are encouraging as community-based approaches to targeting are suppose to exploit information available to community members. However, the results suggest that improvements in information do not fully explain the disproportionate allocation of resources towards connected households; even after controlling for relevant borrower characteristics, connected households are still 10 percentage points more likely to obtain credit from the program.[48] One alternative explanation for this allocation, which could potentially be consequential for the program's sustainability, is favoritism. If that were true, connected households should obtain better loan terms, leading to lower returns for the lender.

1.6.1 Favoritism towards connected households

In order to test for favoritism accounting for unobserved borrower characteristics, I use a sub-sample of 344 households who have ever borrowed from both the program and other local credit sources. I compare differences in initial interest rates and *ex post* returns to the lender for loans obtained from the program with respect to loans from local credit groups for connected households to similar differences for unconnected households, controlling for borrower and lender fixed effects.[49]

households that, despite not obtaining institutional credit, have credit experience from informal loans.

[48]Note that the R-squared from column (6) is considerably lower than that of columns (7)-(9), suggesting that the control variables capture important information explaining the probability of obtaining credit under different allocation criterion. This pattern suggests that household characteristics could be good predictors of uptake of credit from pre-existing sources nut they are not as good in the case of program, which suggests that selection in unobservable characteristics is even more important in the MBVF case.

[49]Such an approach is common in the literature in the context of credit and political connections Khwaja and Mian (2005), testing across monitoring models Shaban (1987), and the study of the role of comparative advantages and taste-based discrimination in agricultural tasks Foster and Rosenzweig (1996).

Comparison local credit groups include production credit groups (PCGs), women's groups, and other village organizations that provide credit. These credit groups and the MBVF program are managed in similar ways: The allocation of credit is decided by community members. However, they differ the way they are funded: The MBVF program is fully funded by the government while local credit groups rely on contributions from group members.[50] The similarities and differences across these sources of credit allow me to focus on two sources of variation: variation in borrower's connection status, which captures the potential political influence; and variation in the origin of the funds, which captures the ability of borrowers to take advantage of their connections.

I focus on initial loan characteristics such as interest rates, term, and size. As repayment frequencies vary across loans, I focus on loan outcomes from a cross section of loans that reached maturity, and were obtained after the implementation of the program. As the recovery rate of loans from the program is 99% in the sample,[51] I measure loan performance as the probability of a delinquent payment and the delinquency rate of the loan. Since differences in loan characteristics may affect repayment, and loan size may reduce administrative costs and interest rates, the main outcome of interest is the *ex post* internal rate of return.

In order to test for favoritism I use the following specification:

$$Y_{kijt} = \alpha_i + \theta_j + \beta \text{Connected}_i \times \text{MBVF}_j + \delta_{vt} + \varepsilon_{kijt} \tag{1.5}$$

The unit of observation is a loan k obtained by household i from lender j in year t. Y_{kijt} denotes the loan outcome or loan characteristic of interest. α_i and θ_j denote households and lender fixed effects. While the analysis is in principle cross sectional, I control for village-specific time-varying shocks by including village-year fixed effects (δ_{vt}). $Connected_i$ and $MBVF_j$ are

[50]These sources of credit have been shown to be helpful in promoting asset growth, consumption smoothing, and occupational mobility through the provision of cash credit to community members in the context of Thailand (Kaboski and Townsend, 2005).

[51]The recovery rate is 96% for local credit groups.

indicators of whether a borrower has pre-program connections with the elites and whether the loan was obtained from the MBVF program. The parameter of interest is β which measures relative performance of loans to connected households from the MBVR program, under the assumption that there were no unobserved shocks differentially affecting program loans corresponding to connected households. This concern is partially assessed by including borrower fixed effects, but this assumption would be violated if, for example, the program modified repayment behavior specific to a type of lender (MBVF or local credit groups) and a type of borrower (connected or unconnected). [52]

Columns (1) to (4) from Table 1.5 present means of loan characteristics and outcomes by type of borrower and lender. Column (8) presents estimates of β corresponding to the specification in equation (1.5), and shows that loans from connected households are relatively larger (22%) and cheaper: The initial interest rate for program loans to connected households is 1.5 percentage points lower than that for loans from local credit groups to connected households, while unconnected households borrow at the same rate regardless of the source. To understand whether better loan conditions relate to favoritism or actually reflect lower risk, Column (8) in Panel B shows that while connected households were less likely to have had a delinquent payment on loans obtained through the program, this difference did not compensate for the preferential interest rates, as delinquency is very low for both sources of credit. As a result, there is a 2-percentage-points decrease in the *ex post* internal rate of return to the lender for MBVF loans to connected households, which accounts for 25% of the average *ex post* internal rate of return for loans from self-funded local credit groups. Note that all the differences arise from differences in loan outcomes for connected households; Columns (5) and (6) show differences in loan outcomes by type of lender for connected and unconnected households, respectively. No significant differences, other than loan size, are detected for unconnected households.

[52] I provide supporting evidence for this assumption in Appendix Table A.5. Columns (4)-(6) test for differential short-run effects of the program on borrowing from credit groups using the rollout of the program; there are no significant effects and the point estimates are not economically meaningful. An explanation of the empirical approach used to obtain these results is deferred to Section 1.7.

1.6.2 Discussion

The results in this section support the hypothesis of favoritism towards connected households in the context of the program in the form of cheaper credit, which is associated with foregone returns to the program. However, the results do not imply that the repayment rate for the program was poor or that it was not profitable, overall. The results do suggest that program loans could have gone to better hands and that the program could have grown faster. For instance, this behavior may explain why, despite its high repayment rate, the program's lending portfolio was not able to grow at the same pace as the Thai economy (Haughton et al., 2014).

Although the evidence in this section is consistent with the notion of costly favoritism, there are other compelling reasons why connected households obtained more credit from the program. First, village fund committee members may have tried to increase employment or stimulate the market for inputs. Second, elected committee members may have different preferences which not necessarily follow a poverty targeting, productive efficiency or risk minimizing criteria. I argue that such a large difference in program participation will be harder to reconcile with alternative preferences other than taste-based discrimination. Third, unobserved application costs may differentially affect unconnected households. While the program relaxed the need for collateral, borrowers were still required to obtain two cosigners and finding a reliable cosigner might be costlier for unconnected households, yet this potential explanation is supportive of the main implication of the results in this section: a community based approach to allocating credit imposes higher program participation costs to unconnected households.

The evidence in this section is meaningful to the extent that unconnected households needed extra-liquidity and selection into the program is mostly explained by supply side constraints. Section 1.7 provides evidence supporting this assumption inspired in the following idea: To the extent that the program favored connected households, other actors in the financial system should be willing to serve unconnected households who want credit. The following section provides evidence of how credit markets reacted to an expansion of credit in the village economy that targeted connected households.

1.7 Program spillovers to unconnected households

The results from the previous section show that the program favored households with connections with the elite and might not have directly reached unconnected households. However, as favoritism is costly, other lenders in the market should be willing to provide loans to disfavored, unconnected households. In this section, I test the empirical relevance of this argument by analyzing whether the supply shock generated by the program indirectly increased credit use by unconnected households. Because institutional lenders are likely to face adjustment costs, I focus the analysis on informal markets which might be flexible enough to quickly respond to the increased in credit supply induced by the program.[53] Analyzing program spillovers is important for two reasons. First, it allows for analyzing the extent to which the resulting program allocation was driven by unconnected households self-excluding from the program or actually being disfavored by program committee members. Second, testing for program spillovers is informative about the role of markets in offsetting targeting errors and about the extent to which resources to improve targeting of social programs may be a first order concern for policy members.

1.7.1 Empirical strategy

The program represented a sudden increase in total lending in the village economy. Figure A.2 shows that there was a spike in aggregate lending within the first two months of the release of the funds from the program which lead to further increase. Following the introduction of low interest loans from the program, aggregate borrowing increased by 24% in the sample villages within a year from the rollout of the program. To determine if there were short-term reactions in credit markets, I exploit monthly variation in the differential rollout of the program

[53] The literature has documented the importance of informal markets in providing resources to households that may not have direct access to formal credit (Kinnan and Townsend, 2012), or were not eligible for social programs (Angelucci and De Giorgi, 2009).

across villages: The resources were released in June 2001 in the first village in the study sample, and the rollout continued until February 2002 for the last village in the dataset (nine months). As this variation is relevant over a short period of time, I restrict the analysis to the 18 months just before and after the program was introduced in each village, and hence the results are only informative of the short-run impacts of the release of the program. Identification of the treatment effects from the rollout of the program is achieved under the assumption that, conditional on household time-invariant characteristics, the rollout of the program was not related to unobserved shocks that determined household decisions to obtain credit. A main concern in this context is the potential coincidence of the program's rollout with different periods in the agricultural cycle. Section 1.7.3 and Appendix Section 2.8 develop a framework to directly test for this threat to identification and discuss other methodological issues.

In order to examine the presence of pre-program trends and the dynamics of the effect of the program, I compute flexible difference-in-differences estimates of the rollout of the program on credit using the following empirical specification (1.6):

$$Y_{ivt} = \alpha_i + \delta_t + \sum_{j=-18, j\neq -1}^{j=18} \beta_j \mathbf{I}[\tau_{vt} = j] + \varepsilon_{ivt} \tag{1.6}$$

where Y denotes total borrowing by household i, in village v, at month t. τ_{vt} denotes time to treatment for each village in a given month. Household fixed effects are denoted by α_i, and δ_t denotes a set of calendar months and year indicators.[54] The coefficients of interest are $\{\beta_j\}_{j=-18}^{18}$, which capture the difference between total average borrowing by households in period $\tau_{vt} = j$ relative to the month preceding release of the funds ($\tau_{vt} = -1$) compared to the difference in total borrowing by households in villages where funds were not released by that month. A

[54]Note that as time to treatment is strongly correlated with survey wave, inclusion of monthly dummies could lead to multicollinearity and failure to identify any meaningful parameter and inability to test for parallel trends. By using calendar month and year fixed effects it is possible to construct a survey-wave-specific intercept and weaken the correlation with the 'time-to-treatment' variable. Future versions of the paper will implement the methods suggested by Borusyak and Jaravel (aper) for this type of problems to test for pre-program trends more formally.

causal interpretation of these parameters relies on the assumption of parallel pre-treatment trends between the villages ($\beta_j = 0, \forall j < 0$) and the absence of post-program, village-specific shocks that may affect borrowing decisions. To approximate the average treatment effect of the rollout of the program over the period of analysis, I also estimate:

$$Y_{ivt} = \alpha_i + \delta_t + \beta Post_{vt} + \varepsilon_{ivt} \tag{1.7}$$

In this equation, $Post_{vt}$ is an indicator that takes the value of 1 in the months following the rollout of the program in each village. The parameter of interest in equation (1.7) is β, which captures the average differences in credit uptake before and after the release of program funds across households from villages that experienced the release of the funds in different periods.

A comment regarding inference should be made: I use mainly variation across 16 villages in the timing of the rollout of the program to identify intention-to-treat effects of the program, and the scarce number of villages poses a threat both to power and accurate inference. I present standard errors clustered at the household level to account for serial correlation. To account for within village correlation of error terms in the context of a small number of clusters, the regression tables report p-values from the wild bootstrap-t procedure suggested by Cameron and Miller (2015) imposing the null hypothesis of no effect. However, this approach tends to have low power and lead to conservative inference.[55]

1.7.2 Results

I find that unconnected households indirectly benefited from the program through informal local credit markets, mostly from relatives. Figure 1.3 presents flexible difference-in-difference estimates corresponding to equation (1.6) and shows that there was an increase in borrowing from informal lenders by unconnected households, and no effect for connected house-

[55] As discussed by Cameron and Miller (2015) most available corrections for small number of clusters lead to appropriate acceptance rates, but they have reduced power. This is a concern in this paper as the number of cross-section observations is small.

holds. Figure 1.4 shows that the effect is mostly driven by loans from relatives, usually at null interest rates–average interest rate is 9%, but median interest rate is 0–.[56] Table 1.6 presents average treatment effects by connections with the elites and shows that the program lead to a 30% increase in informal debt in the case of unconnected households.

The results reported in the previous paragraph are consistent with evidence of re-lending. Appendix table A.6 shows that the probability of lending to other households increased by 2 percentage points in the case of connected households (12% of pre-program average), as a result of the rollout of the program. Event-study estimates show that there was a surge in total lending for connected households within two months of the rollout of the program (see Appendix figure A.3), yet these effects are imprecisely estimated.

Moreover, kinship networks were not the only margin of adjustment. Appendix Figure 1.5 shows that unconnected households borrowed more from BAAC, the state-owned bank, in some periods following the rollout of the program. These results constitute only suggestive evidence of spillovers as the average effect is not significant, though economically meaningful (See Appendix Table A.5).

Overall, the program had little effect on total borrowing for connected households. Appendix Table A.4 and Appendix figure A.4 present estimates of the impact of the rollout of the program on the probability of holding any loan and total debt from any source, by connections with the elites. The figure shows that the program barely increased access to credit for connected households despite providing them with over twice as much resources than connected households. This finding is not surprising as connected households had higher access to credit even before the program.

Despite spillovers and general equilibrium effects driving increases in non-program borrowing for unconnected households, the magnitudes are not big enough to fully compensate the differences in total borrowing form the program. Back-of-the-envelop calculations suggest that the effects on non-program borrowing for unconnected households only account from one-

[56]However, interlinked transactions in the kinship network may make up for zero interest rates on loans.

third of the differences in borrowing from the program between connected and unconnected households.[57] The result shows that unconnected households needed liquidity and suggest that the allocation of loans from the program was not explained by self-exclusion. Unconnected households were less likely to obtained credit from the program and when they did, they obtained less money, suggesting that other lenders in the system were helpful in reallocating the resources towards disfavored households.

1.7.3 Threats to identification, robustness, and attrition

The assumption that the rollout of the program was exogenous with respect to credit decisions is central to the identification strategy in the preceding section. While the flexible difference-in-difference estimates show that there were no differential pre-program trends, it is not clear that there were no post-program, village-specific shocks that may have affected credit decisions. Although monthly fixed effects control for seasonality, it could be the case that the funds were differentially released in periods in which higher activity in the credit market was expected. For instance, villages with earlier implementation benefited from the program during planting season, but villages with delayed implementation received the funds at the end of harvest season. If households decided to finance operations in a particular season, the estimates from the preceding difference-in-difference approach could also capture the effect of the agricultural cycle on credit. In Appendix Section A.3, I discuss in detail a placebo exercise designed to test if the results were driven by village-specific, seasonal patterns confounded with the rollout of the program. Concretely, I use observations corresponding to survey waves up to a year before the program was implemented in the first village ($\tau_{v,t} \in [-36, -6)$), and normalize τ, the variable representing time-to-treatment, to be between -12 and 17 (the base category is -1), such that the calendar months in which the funds were actually released coincide with the ones in the placebo exercise. I then estimate equation (1.6) in this sample and compare the results from the placebo

[57] This result is obtained from adding the effect on borrowing from relatives (THB 416) and from BAAC (1,018) for unconnected households and dividing it by the difference between the effect of the rollout of the program on program borrowing for connected households (THB 7,092) and unconnected households (THB 2,583)

sample to those reported in this paper. There are no significant effects in the placebo exercise.

Regarding attrition, I provide replications of the main difference-in-difference results presented in this paper for the 509 households that were interviewed in all 172 rounds of the survey. Results are not sensitive to attrition (See Appendix Section A.3). Regarding potential noise in the measure of connections, I replicate the main tables of the paper using an index of connection with the elites, the computation of which is based on the first principal component of the correlation matrix of connection with elites through all possible socioeconomic interactions. All results hold under both approaches (see Appendix Tables A.18 and A.19).

1.8 Concluding remarks and discussion

Community-based approaches to targeting public resources are increasingly popular in the policy world. Despite that, little is known regarding the performance of these approaches in market-driven environments such as credit. This paper brings together two central debates in development economics: the delivery of public resources through local democratic organizations and the provision of affordable credit to poor, high-productivity households. The results in this paper highlight the limitations of a subsidized community-based credit program to deliver credit to poor, high productivity households. Consistent with the traditional concern of resource capture in the literature that studies the decentralization of public programs to community members (Bardhan and Mookherjee, 2005), resources from the program were disproportionately allocated to households with baseline business connections with local elites.

These results are partially explained by the role of information. After controlling for demographic and productive characteristics as well as credit history, the correlation between connections and program participation reduces, yet it is still strong. This result suggests that the cost of obtaining relevant borrower information was higher for unconnected households and has important policy implications in contexts in which attributes for beneficiaries are hard to observe. The extent to which community-based targeting approaches lead to better targeting will depend on how connected are potential beneficiaries. Concretely, if poor,high-productivity households are socioeconomically isolated, even in the absence of rent-seeking behavior they may be less likely to be targeted. This result complements evidence showing how village network characteristics explain heterogeneity in targeting errors from a community-based cash transfer program(Alatas et al., 2012).

This paper also documents evidence of favoritism in a context of transparent elections of village fund committee members and speaks to the debate regarding the delivery of public resources through local democratic organizations. While the expectation was that transparent elections would ensure accountability, the results in this paper suggests that elections politicized the allocation of resources. The results are consistent with the theoretical prediction that decentralization may lead to regressive allocations when policies are financed through government grants instead of user contributions (Bardhan and Mookherjee, 2006a), as is the case of the MBVF program, and with cross-village studies documenting favoritism and clientelism (Asher and Novosad, 2017; Anderson et al., 2015). Overall, the results suggest that differences in connections to the local elite across households capture different in costs of accessing to public resources. These costs are related to information transmission but also to favoritism and are consequential in terms of equity, productive efficiency and program sustainability.

The results contrasts sharply with evidence in the context of community-based targeting of cash-transfer programs (Alatas et al., 2012) but is consistent with evidence of favoritism towards politically connected firms and credit from state-owned banks (Khwaja and Mian, 2005). The intuition for this result is that, as opposed to targeting cash transfers, the allocation of credit not only involves information regarding poverty but also productivity and repayment. Information regarding poverty is more likely to be objective and common knowledge to the community as a whole. Community members may use observable characteristics that describe a poor household and may not need to interact directly in order to figure out who is poor. In contrast, information regarding productivity and repayment requires direct economic interactions and thus may increase the incentives for moral hazard behavior.

A first order concern is that of how to effectively use the information available to community members and simultaneously prevent rent-seeking behavior in community-based approaches. One way could be by fostering self-funded credit groups, as opposed to creating village funds with subsidized resources. This is already a popular policy approach backed with encouraging evidence of its effects both on household productive behavior (Kaboski and Townsend, 2005; Deininger, 2013) and in relieving households from usurers (Hoffmann et al., 2017). Research testing whether there are social barriers preventing poor, high-productivity households from participating in these groups would shed light regarding the effectiveness of this approach to alleviate poverty. Moreover a more careful comparison of the mechanisms driving selection into credit across different policy-relevant implementation approaches –i.e., CBT, self-help groups and traditional microfinance– would provide insights towards future policy directions. An alternative way is to provide monetary incentives for accurate information (Hussam et al., 2017), however the implementation of these incentives may require bureaucracy which is precisely what CBT approaches are trying to avoid.

This study also speaks to the importance of understanding the interactions of public policy efforts with markets, and political economy factors in a general equilibrium framework. In particular, this paper contributes with novel evidence showing that credit markets may offset potential targeting errors. While evidence of spillovers from large scale programs towards mistargeted households may suggest that targeting should not be a first order concern as markets may deliver resources to the intended destination, the relevant question is the price mistargeted households have to pay in order to benefit from public resources. This study finds that other lenders in the village financial system and kinship networks are important in indirectly delivering results to households lacking of connections with local leaders. While the former involved higher interest rates than those from loans from the program, the latter may imply interlinked transactions which may be costly for either the borrower or the lender. These costs may ultimately determine if targeting should be a first or second order issue in public policy.

Finally, this paper provides evidence that aids in interpreting the results from the impact evaluation of the MBVF program. First, Kaboski and Townsend (2012) find increases in consumption and income growth with no effect on investment. Ongoing work by Breza et al. (2017) document heterogeneous effects of credit from the MBVF on investment, driven by heterogeneity in productivity. My results provide a bridge between these studies by showing that credit was inefficiently allocated and documenting the mechanisms leading to that allocation. Second, other studies analyzing whether the program reached poor households suggest that resources were directed towards the poor, based on inter-village comparisons (Haughton et al., 2014; Menkhoff and Rungruxsirivorn, 2011). By using socioeconomic networks data, the results from this paper suggest that cross-village comparisons hide substantial asymmetries in access to resources from the program, which only a detailed intra-village analysis is able to capture.

1.9 Figures

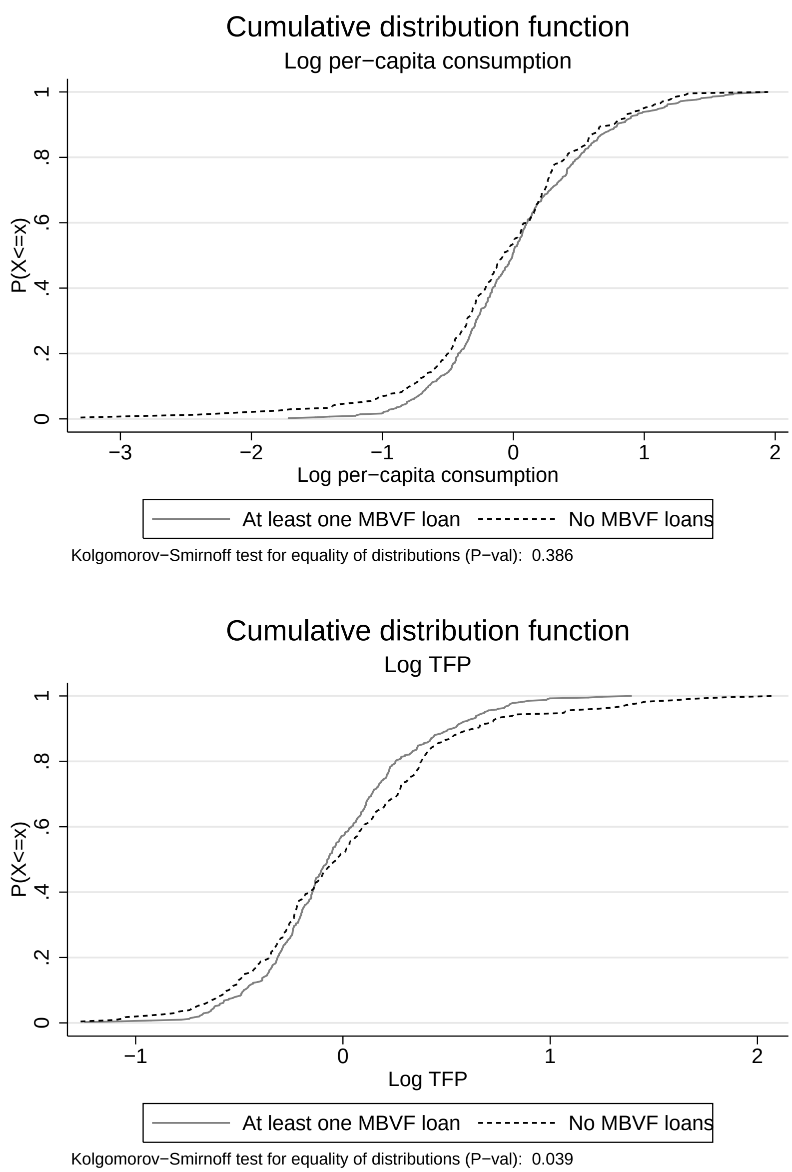


Figure 1.1. Cumulative Distribution Function of Baseline Per-capita Consumption and Productivity

Note: The top figure plots the cumulative distribution function (CDF) of log per-capita consumption, measured at baseline, for households with access to credit from the program (62%) and households who didn't obtain credit from the program (38%) during the first two years of its implementation. The bottom figure plots a similar comparison for the CDF of log total factor productivity. Both variables are centered with respect to the village mean in order to perform within village comparisons. Per-capita consumption is measured as the total per-capita expenditure in consumption goods for the 12 months preceding the introduction of the program. Baseline total factor productivity is estimated using capital and labor elasticities corresponding to a value-added production function estimated as in Ackerberg et al. (2015).

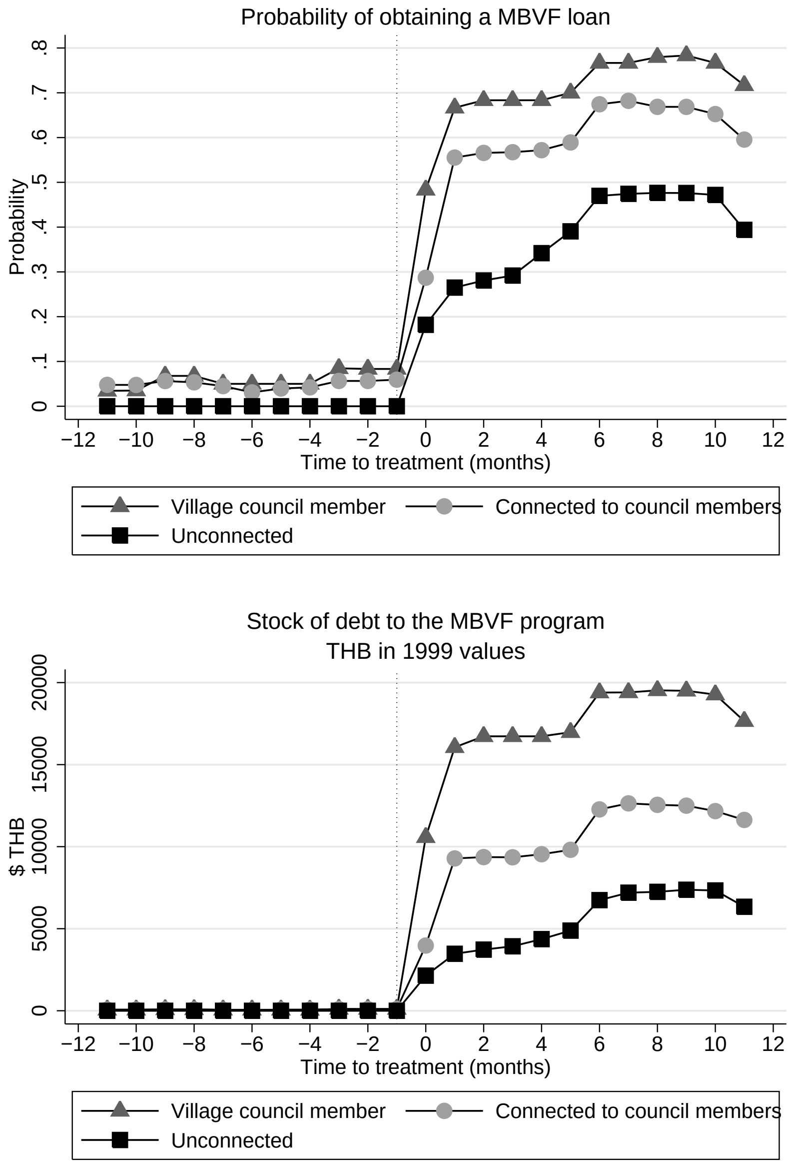


Figure 1.2. Access to Credit and Connections with the Elites

Note: The figure depicts the probability of holding an outstanding loan from the the Village Fund program ((top panel) and the average gross stock of debt from the program (bottom panel) for the 12 months preceding and following the implementation of the program. Each symbol denotes the mean for each category in a given month. The dotted line denotes the period preceding the release of the program's funds $\tau_{v,t} = -1$. Village council member: .households in which at least one member is either the village head or on the village council during pre-program periods. Connected to council members: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

.

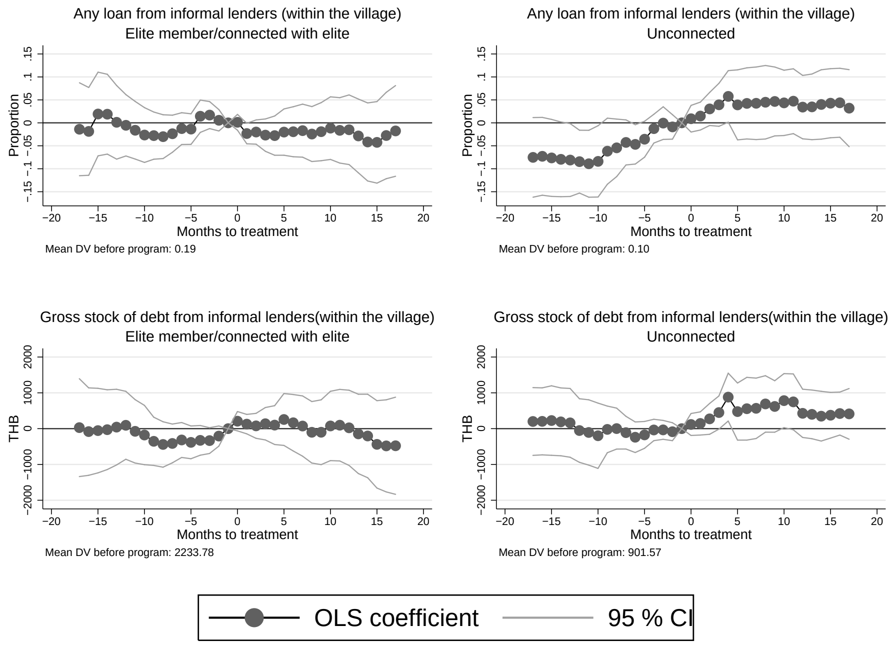


Figure 1.3. Short-term Effects of on Credit From Informal Lenders

Note: The figure depicts OLS point estimates from a flexible difference-in-difference model following equation 1.6. Each dependent variable was regressed on household fixed effects, calendar month and year fixed effects, an a set of indicators that denote time to treatment. Each dot represents the coefficient associated with each of these indicators. The base category corresponds to the period preceding the first month of operation of the fund: $\tau_{vt} = -1$. Confidence intervals are constructed using standard errors clustered at the household level, to account for serial correlation. Informal lenders include both relative and non-relative personal lenders.

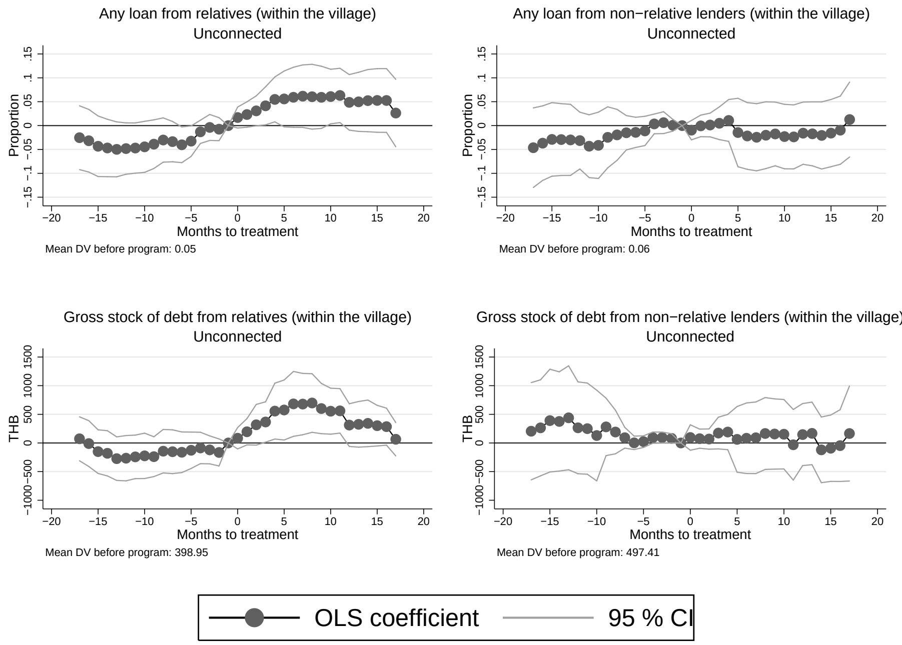


Figure 1.4. Short-term Effects on Credit From Relatives for Unconnected Households

Note: The figure depicts OLS point estimates from a flexible difference-in-difference model following equation 1.6. The left-hand panel presents estimates for loans from relatives, while the right-hand panel shows estimates for loans from local non-relative lenders. Each dependent variable was regressed on household fixed effects, calendar month and year fixed effects, and a set of indicators that denote time to treatment. Each dot represents the coefficient associated with each of these indicators. The base category corresponds to the period preceding the first month of operation of the fund: $\tau_{vt} = -1$. Confidence intervals are constructed using standard errors clustered at the household level, to account for serial correlation. The estimating sample includes only unconnected households.

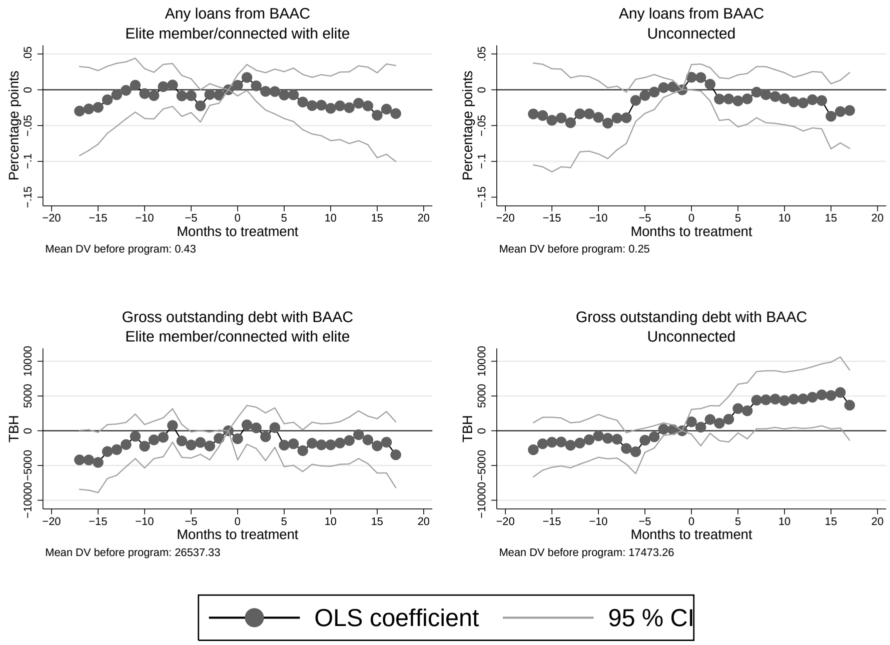


Figure 1.5. Short-term Effects on Non-program Institutional Credit (BAAC)

Note: The figure depicts OLS point estimates from a flexible difference-in-difference model following equation 1.6. The top panel reports coefficients for the probability of holding any outstanding loan from the Bank of Agriculture and Agricultural Cooperatives (BAAC). The bottom panel presents results for the stock of outstanding debt with BAAC. Each dependent variable was regressed on household fixed effects, calendar month and year fixed effects, and a set of indicators that denote time to treatment. Each dot represents the coefficient associated with each of these indicators. The base category corresponds to the period preceding the first month of operation of the fund: $\tau_{vt} = -1$. Confidence intervals are constructed using standard errors clustered at the household level, to account for serial correlation.

1.10 Tables

Table 1.1. Program Participation, Poverty and Productivity

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log per-capita consumption (N=660)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Any MBVF loan	0.135**	0.218*	0.116**	0.108*	0.057	0.012
	(0.056)	(0.124)	(0.049)	(0.057)	(0.068)	(0.094)
Panel B: Total factor productivity (logs) (N=637)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Any MBVF loan	-0.052	0.099**	0.050	-0.029	-0.152***	-0.083
	(0.044)	(0.048)	(0.034)	(0.048)	(0.042)	(0.068)
Panel C: Asset turnover (log revenues/assets) (N=666)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Any MBVF loan	0.182	0.645***	0.287	0.027	-0.105	-0.081
	(0.122)	(0.179)	(0.176)	(0.115)	(0.137)	(0.128)
Panel D: Profitability margin (profits/revenues) (N=674)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Any MBVF loan	-0.049**	-0.076	-0.044	-0.055***	-0.032***	-0.009
	(0.021)	(0.046)	(0.028)	(0.015)	(0.007)	(0.006)
Panel E: Total factor productivity (logs)-only baseline data (N=637)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Any MBVF loan	-0.001	0.117**	0.046	-0.005	-0.070*	-0.036
	(0.036)	(0.051)	(0.032)	(0.033)	(0.037)	(0.067)
Panel F: Total factor productivity (logs)-Dynamic Panel (N=629)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Any MBVF loan	-0.110*	0.141**	0.025	-0.093*	-0.150	-0.277***
	(0.065)	(0.069)	(0.045)	(0.052)	(0.092)	(0.101)

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents within-village comparisons of program beneficiaries and non-beneficiaries. Column (1) presents coefficients corresponding to regressions of baseline characteristics on an indicator of whether a household obtained a loan from the program during the first two years following the program implementation in each village, and village fixed effects. Columns (2)-(6) present results for equivalent quantile regressions. The bandwidth used for the estimation of quantile regressions was selected using Hall-Sheather's method. Robust standard errors are presented in parentheses. Panel A reports results for baseline per-capita consumption (in logs). Baseline per-capita consumption is measured as total expenditures during the 12 months preceding the implementation of the program. Panel B reports results for baseline log total factor productivity estimates recovered using capital and labor elasticities corresponding to a value-added production function estimated as in Ackerberg et al. (2015). Panel C presents results for baseline asset turnover ratio (in logs) computed as the average ratio of total revenues over a calendar year divided by the average stock of fixed assets in each household, over the two calendar years preceding the program's rollout (1999-2000). Panel D presents estimates for baseline profitability margins measured as the average ratio of net revenues (net of costs of purchased inputs outside the household) to gross revenues in a given year. Panel E presents results for baseline log total factor productivity estimates recovered using capital, labor elasticities corresponding to a model estimated using only pre-program data. Panel F presents results for productivity computed using a dynamic panel estimation approach corresponding to a gross-revenue function.

Table 1.2. Difference with Means-Testing Criterion

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log per-capita consumption (N=311)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by MT	0.499***	0.477***	0.377***	0.411***	0.484***	0.490***
	(0.065)	(0.156)	(0.044)	(0.074)	(0.057)	(0.094)
Panel B: Total factor productivity (logs) (N=309)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by MT	-0.037	-0.108*	-0.089**	-0.039	-0.024	-0.012
	(0.055)	(0.065)	(0.038)	(0.065)	(0.068)	(0.094)
Panel C: Asset turnover (log revenues/assets) (N=327)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by MT	-0.941***	-0.597*	-0.785***	-1.161***	-1.074***	-1.193***
	(0.186)	(0.323)	(0.166)	(0.121)	(0.146)	(0.245)
Panel D: Profitability margin (profits/revenues) (N=329)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by MT	-0.146***	-0.226***	-0.166***	-0.160***	-0.083***	-0.010
	(0.039)	(0.041)	(0.045)	(0.024)	(0.026)	(0.014)
Panel E: Total factor productivity (logs)-Revenue function dynamic panel (N=305)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by MT	-0.233**	-0.085	-0.075	-0.207***	-0.367***	-0.411***
	(0.091)	(0.085)	(0.053)	(0.060)	(0.119)	(0.079)

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents within village comparisons between households who obtained loans from the program during the first two years of its implementation but would have been excluded according to a means-testing criterion and households who were excluded from the program but would have been offered a loan by a means-testing criterion. Column(1) presents coefficients corresponding to regressions of baseline characteristics on an indicator of whether a household was reached by the Village Fund, but would have been excluded by a MT criterion, and village fixed effects. The omitted category (comparison group) is comprised of the households who would have been included by MT but were excluded from the program. Columns (2)-(6) present results for equivalent quantile regressions. The bandwidth used for the estimation was selected using Hall-Sheather's method. Robust standard errors are presented in parentheses. Panel A reports results for baseline per-capita consumption (in logs). Baseline per-capita consumption is measured as total expenditures during the 12 months preceding the implementation of the program. Panel B reports results for baseline log total factor productivity estimates recovered using capital and labor elasticities corresponding to a value-added production function estimated as in Ackerberg et al. (2015). Panel C presents results for baseline asset turnover ratio (in logs) computed as the average ratio of total revenues over a calendar year divided by the average stock of fixed assets in each household, over the two calendar years preceding the program's rollout (1999-2000). Panel D presents estimates for baseline profitability margins measured as the average ratio of net revenues (net of costs of purchased inputs outside the household) to gross revenues in a given year. Panel E presents results for baseline log total factor productivity estimates recovered using capital, labor, and intermediate inputs elasticities corresponding to a gross-revenue function estimated using a dynamic panel estimation approach.

Table 1.3. Differences with Credit-Scoring Criterion

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log per-capita consumption (N=273)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by a credit score criterion	0.052	0.317	0.092	-0.083	-0.191**	-0.341**
	(0.103)	(0.212)	(0.076)	(0.071)	(0.086)	(0.142)
Panel B: Total factor productivity (logs) (N=276)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by a credit score criterion	-0.116*	0.070*	0.022	-0.138*	-0.172***	-0.152***
	(0.060)	(0.039)	(0.043)	(0.075)	(0.037)	(0.050)
Panel C: Asset turnover (log revenues/assets) (N=285)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by a credit score criterion	-0.021	0.127	-0.039	-0.230	-0.300*	-0.318***
	(0.176)	(0.285)	(0.193)	(0.155)	(0.153)	(0.093)
Panel D: Profitability margin (profits/revenues) (N=290)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by a credit score criterion	0.016	0.024	-0.039	-0.004	-0.013*	-0.009**
	(0.030)	(0.081)	(0.036)	(0.023)	(0.006)	(0.004)
Panel E: Total factor productivity (logs)- Revenue function dynamic panel (N=305)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by a credit score criterion	-0.164	0.202**	0.038	-0.117	-0.229*	-0.522***
	(0.100)	(0.093)	(0.090)	(0.092)	(0.137)	(0.133)

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents within village comparisons between households who obtained loans from the program during the first two years of its implementation but would have been excluded according to a credit score (CS) criterion and households who were excluded from the program but would have been offered a loan by a CS criterion. Column(1) presents coefficients corresponding to regressions of baseline characteristics on an indicator of whether a household was reached by the Village Fund, but would have been excluded by a CS criterion, and village fixed effects. The omitted category (comparison group) is comprised of the households who would have been included by CS but were excluded from the program. Columns (2)-(6) present results for equivalent quantile regressions. The bandwidth used for the estimation was selected using Hall-Sheather's method. Robust standard errors are presented in parentheses. Panel A reports results for baseline per-capita consumption (in logs). Baseline per-capita consumption is measured as total expenditures during the 12 months preceding the implementation of the program. Panel B reports results for baseline log total factor productivity estimates recovered using capital and labor elasticities corresponding to a value-added production function estimated as in Ackerberg et al. (2015). Panel C presents results for baseline asset turnover ratio (in logs) computed as the average ratio of total revenues over a calendar year divided by the average stock of fixed assets in each household, over the two calendar years preceding the program's rollout (1999-2000). Panel D presents estimates for baseline profitability margins measured as the average ratio of net revenues (net of costs of purchased inputs outside the household) to gross revenues in a given year. Panel E presents results for baseline log total factor productivity estimates recovered using capital, labor, and intermediate inputs elasticities corresponding to a gross-revenue function estimated using a dynamic panel estimation approach.

Table 1.4. Access to Credit, Connections, and Borrower Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	**DV: Household obtained at least one loan from the MBVF**						**Any formal loan**	**MT**	**CS**
Relationship with village council members									
Connected through socioeconomic interactions	0.185***	0.141***	0.111**	0.097**	0.092*		0.085**	0.074	-0.009
	(0.043)	(0.046)	(0.048)	(0.048)	(0.049)		(0.038)	(0.047)	(0.045)
Village council member						0.164**			
						(0.070)			
Directly connected to a council member (interactions)						0.079			
						(0.050)			
First-degree relative of council member						0.061			
						(0.057)			
Network centrality									
Degree (# of links)		0.010***	0.005	0.003	0.004	0.003	0.010***	-0.015***	-0.000
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Household demographic characteristics									
Number of males (15-64)			-0.054	-0.059*	-0.065*	-0.064*	0.026	-0.000	0.001
			(0.034)	(0.034)	(0.034)	(0.035)	(0.029)	(0.034)	(0.033)
Number of females (15-64)			-0.053	-0.051	-0.077**	-0.079**	-0.019	0.035	0.054
			(0.037)	(0.036)	(0.037)	(0.038)	(0.028)	(0.038)	(0.039)
Number of household members			0.046***	0.043**	0.045**	0.045**	0.017	0.004	-0.016
			(0.017)	(0.017)	(0.018)	(0.018)	(0.016)	(0.018)	(0.019)
Average years of schooling			0.023*	0.015	0.019	0.018	0.042***	-0.090***	0.059***
			(0.013)	(0.013)	(0.014)	(0.014)	(0.011)	(0.012)	(0.011)
Household head's age			-0.002	-0.001	-0.003	-0.003	-0.005*	-0.003	0.000
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Average age			-0.001	-0.001	0.001	0.001	0.003	-0.000	-0.002
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Household head is a male			0.037	0.036	0.038	0.027	0.023	0.031	0.167***
			(0.046)	(0.045)	(0.047)	(0.048)	(0.036)	(0.046)	(0.048)
Sources of revenue (share of total)									
Wage labor			0.265**	0.210	0.164	0.151	0.262**	0.238	0.073
			(0.128)	(0.128)	(0.142)	(0.143)	(0.118)	(0.148)	(0.147)
Family business			0.258**	0.225*	0.209	0.196	0.217*	0.005	0.038
			(0.130)	(0.130)	(0.142)	(0.142)	(0.114)	(0.149)	(0.148)
Fishing/shrimping			0.427*	0.356	0.428*	0.410	0.503**	-0.222	0.496*
			(0.244)	(0.244)	(0.253)	(0.251)	(0.196)	(0.240)	(0.260)
Livestock			0.252*	0.176	0.174	0.177	0.431***	0.106	0.446***
			(0.142)	(0.142)	(0.155)	(0.155)	(0.130)	(0.164)	(0.159)
Agriculture			0.442***	0.333*	0.258	0.254	0.549***	-0.168	-0.068
			(0.167)	(0.171)	(0.185)	(0.186)	(0.151)	(0.191)	(0.186)
Credit history									
Avg. baseline delinquency			-1.008***	-1.040***	-1.009***	-1.001***	0.206	0.096	
			(0.293)	(0.304)	(0.317)	(0.312)	(0.251)	(0.211)	
Avg. baseline income volatility			0.038*	0.029	0.039	0.037	0.052***	-0.062**	
			(0.022)	(0.022)	(0.024)	(0.024)	(0.020)	(0.026)	
Pre-program access to institutional credit				0.182***	0.152**	0.146**		0.016	
				(0.058)	(0.060)	(0.060)		(0.058)	
Household productivity									
Estimated household total factor productivity					-0.088*	-0.084*	-0.092**	0.095*	0.030
					(0.049)	(0.049)	(0.038)	(0.049)	(0.047)
Observations	649	649	616	616	587	587	587	587	608
Adjusted R-squared	0.11	0.12	0.14	0.16	0.15	0.15	0.39	0.23	0.19
Within R-squared	0.03	0.04	0.07	0.09	0.08	0.08	0.17	0.16	0.10

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: MT: Means testing. CS: Credit Score. The table presents within-village comparisons of the probability of obtaining a MBVF loan between connected households and unconnected households under several specifications (Columns (1) to (3)). Column(1) presents OLS coefficients from cross-section regressions of an indicator of whether a household obtained a loan from the program within two years of its implementation, controlling for village fixed effects. Column(2) controls for degree centrality in the socioeconomic network. Column(3) includes baseline household characteristics and Column (4) controls for baseline access to credit and Column(5) includes estimated productivity. Column (6) replicates the approach in Column (3) breaking down connections with the elite by type of connection. Columns(7) to (9) replicate the estimations for the probability of having held any institutional loan before the program (Column (7)), the probability of being targeted by the means-testing criterion (Column (8)), and the probability of being targeted by the credit-score criterion (Column (9)). Baseline access to institutional credit is an indicator of whether a household had any loan from either formal lenders or quasi-formal lenders. The delinquency rate is computed as the share of loans in which a household held any delinquent payments, and is computed based on repay information regarding loans from all type of lenders, including loans from relatives and informal lenders. Robust standard errors are reported in parentheses. Income volatility: log of the coefficient of variation of monthly income computed over all the survey waves preceding the program. Connected to council members: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected households: households without any direct connection with members of the village council.

Table 1.5. Differences in Loan Outcomes by Connections with the Elites

Panel A: Loan characteristics

	Means				Difference (MBVF-CG)		Difference-in-differences		
	Connected (N=231)		Unconnected (N=83)		Connected (N=231)	Unconnected (N=83)	All borrowers N=344		
	MBVF	Local credit groups (CG)	MBVF	Local credit groups (CG)	(1)-(2)	(3)-(4)	(5)-(6)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Initial interest rate (annual)	0.054	0.078	0.059	0.067	-0.0212***	-0.0065	-0.0150***	-0.0124***	-0.0120***
					(0.003)	(0.004)	(0.005)	(0.004)	(0.004)
Term (months)	11	12	11	13	-0.2714	-0.9951*	0.7482	0.8483	0.7822
					(0.242)	(0.539)	(0.625)	(0.596)	(0.604)
Loan size (TBH-1999 prices)	15175	4029	11659	3992	11,168***	8,550***	2,579***	2,179***	
					(375.973)	(706.135)	(750.099)	(739.294)	

Panel B: Loan outcomes

	Means				Difference (MBVF-CG)		Difference-in-differences		
	Connected (N=231)		Unconnected (N=83)		Connected (N=231)	Unconnected (N=83)	All borrowers N=344		
	MBVF	Local credit groups (CG)	MBVF	Local credit groups (CG)	(1)-(2)	(3)-(4)	(3)-(6)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any delinquent payment	0.008	0.017	0.003	0.000	-0.0117**	0.0040	-0.0157***	-0.0095*	-0.0095*
					(0.005)	(0.003)	(0.005)	(0.005)	(0.005)
Delinquent payments as a share of due payments	0.006	0.010	0.002	0.000	-0.0063**	0.0020	-0.0082**	-0.0049	-0.0048
					(0.003)	(0.001)	(0.003)	(0.003)	(0.003)
Any loan extension	0.470	0.400	0.372	0.336	0.0206	0.0239	-0.0034	-0.0233	-0.0211
					(0.022)	(0.038)	(0.044)	(0.042)	(0.041)
Ex post internal rate of return (annual)	0.060	0.077	0.068	0.059	-0.0183***	0.0081	-0.0263***	-0.0243***	-0.0236***
					(0.004)	(0.007)	(0.008)	(0.007)	(0.007)
Borrower fixed effect					YES	YES	YES	YES	YES
Lender fixed effect					NO	NO	NO	YES	YES
Village -year trends					NO	NO	NO	YES	YES
Weights for loan size					NO	NO	NO	NO	YES
Observations					5,193	1497	6,690	6,690	6,690

$***p < 0.01, **p < 0.05, *p < 0.1$

Notes: Columns (1)-(2) present raw means for connected household loans obtained from the MBVF program (1) and other local credit groups (2). Columns (3)-(4) present raw means for unconnected household loans obtained from the MBVF program (3) and other local credit groups (4). Columns (5) and (6) present differences in loan outcomes and characteristics across lenders for connected and unconnected households, respectively. Both differences control for borrower fixed effects. Columns (7)-(9) present difference-in-differences estimates under several specifications (First difference: Lender. Second difference: Connection status). Each coefficient captures the difference in differences in attributes of loans obtained by connected households from the program compared to loans from local credit groups, and similar differences for unconnected households. Column (7) presents estimates that only control for borrower and lender fixed effects. Column (8) includes a full set of village-year dummies. Column (9) replicates the estimates presented in Column (8) weighting each observation by loan size. Standard errors are clustered at the household level to account for correlation in loan outcomes corresponding to a single borrower. The sample corresponds to loans obtained after the rollout of the program by a set of 344 households who borrowed from both sources of credit at some point. Local credit groups include production credit groups, women's groups, and other loans from local non-bank institutions. Connected to council members: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected households: households without any direct connection with members of the village council.

Table 1.6. Short-run Program Effects on Credit from Informal Lenders

Panel A: Any loan from informal lenders

	Connected			**Unconnected**		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Any informal	Relatives	Non-relatives	Any informal	Relatives	Non-relatives
$Post_{vt}$	-0.007	-0.004	-0.005	0.047**	0.051***	0.002
	(0.014)	(0.011)	(0.012)	(0.022)	(0.019)	(0.012)
	[0.796]	[0.664]	[0.824]	[0.168]	[0.020]	[0.936]
Observations	13,212	13,212	13,212	6,948	6,948	6,948
R-squared	0.665	0.667	0.637	0.575	0.539	0.601
Baseline DV mean	0.150	0.0680	0.111	0.0815	0.0507	0.0498
Clusters	367	367	367	193	193	193

Panel B: Gross stock of debt with informal lenders

	Connected			**Unconnected**		
	(1)	(2)	(3)	(4)	(5)	(6)
	Any informal	Relatives	Non-relatives	Any informal	Relatives	Non-relatives
$Post_{vt}$	336.651*	124.218	240.688*	655.017***	555.634***	108.889
	(195.463)	(111.706)	(141.390)	(230.061)	(204.585)	(110.267)
	[0.116]	[0.196]	[0.172]	[0.120]	[0.008]	[0.816]
Observations	13,212	13,116	13,075	6,948	6,868	6,948
R-squared	0.672	0.669	0.702	0.607	0.604	0.597
Baseline DV mean	1540	554.8	998.5	865.2	398.1	472.3
Clusters	367	367	366	193	193	193

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on borrowing from informal lenders, by connectedness with the local elites. Informal lenders include personal money lenders and relatives in the village. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (1.7)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Panel A reports results for probability of holding a loan and Panel B shows results for the gross stock of debt (winsorizing the top 1% of observations). Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

1.11 Acknowledgements

Chapter 1, is currently being prepared for submission for publication of the material. Vera Cossio, Diego ”Targeting credit through community members”.

Chapter 2

Dependence or Constraints? Cash Transfers and Female Labor Supply

Abstract

In this paper I examine the extent to which cash-transfer programs may unintendedly boost female employment. Using quasi-experimental variation in the eligibility for a conditional cash transfer program in Bolivian public schools, I study the effects of the program on labor supply for adults from households of eligible children. In a context of high pre-program school attendance and massive gender disparities in adult labor-force participation, I find that the program increased work hours by 8% in the case of adult females from households of eligible children. Three pieces of evidence suggest this increase is explained by fixed costs to work and limited access to institutional credit. First, estimating treatment effects along the distribution of work hours, I find that effects are concentrated on the extensive margin of work. Second, using cross-municipality variation in the pre-program number of branches of financial institutions, I find that the effect is higher for adult females in municipalities with a lower number of per-capita branches. Finally, the program stimulated entrance into self-employment. The results suggest that in a context of fixed costs to work and limited access to credit, two characteristics of developing countries, even small transfers can increase labor force participation.

2.1 Introduction

Cash transfers are common tools for tackling poverty, both in developed and developing countries. While these programs show welfare-increasing effects in many dimensions (Fiszbein et al., 2009), a main concern is whether these types of programs can have negative effects on labor supply and create dependence, leading to a trade-off between immediate poverty alleviation and long-term poverty reduction. Studies from cash welfare programs in developed countries suggest that there is a negative effect on labor supply (Hoynes (1996) and Hoynes and Schanzenbach (2012)), consistent with a neoclassical model of labor supply. However, the literature analyzing the effects of cash transfer programs on labor supply in developing countries systematically fails to find significant treatment effects (Skoufias and Maro (2008), Alzua et al. (2013), Banerjee et al. (2015) and, de Brauw et al. (2015)). Moreover, recent evidence on unconditional cash transfers to groups of young potential entrepreneurs finds increases in work hours due to the program Blattman et al. (2014). Understanding which features of the process of development explain the divergence in results will reconcile the empirical evidence and shed light on the salience of the poverty alleviation-dependence trade-off.

This paper uses the staggered timing and eligibility criteria of a conditional cash transfer program (CCT) in Bolivia to estimate the causal impact of a positive income shock on adults' labor supply through a difference-in-difference approach. The program provided 200 Bolivianos (approximately 25 U.S. dollars) per year to children in Bolivian public schools conditional on 80% attendance during the school year. Using 8 years of Bolivian household surveys, I construct a pooled cross-section dataset of children in public schools in Bolivia[1]. Exploiting the variation in eligibility across school grades and the rollout of the program, I compare changes in work outcomes before and after introduction of the program for adults whose children were enrolled in eligible grades with changes in work outcomes for adults whose children were not beneficiaries of the program.

[1] This represents 90% of children enrolled in school during the year preceding the program.

I find that the program increased the probability of working by 4 percentage points and increased work hours by 2.5 units for adult females (heads of households or their spouses). These effects are small; they represent increases of 6% and 9% with respect to the baseline mean[2], respectively. I find that 90% of this effect comes from adult females whose children were likely to attend to school even in the absence of the program and were not affected by the condition component of the program. This result suggests that a shift in the budget set induced by an income shock dominates potential increases in the availability of time induced by the condition component of the CCT[3]. The results are robust to a variety of specifications and are consistent with an economy characterized by a large, stagnant gender gap in employment[4]

To understand why these apparently unusual positive elasticities appear in the context of a developing country, I outline a simple theoretical framework which predicts the result found in my empirical approach and derive additional predictions which I then take to the data. I do so by drawing on a traditional idea behind the process of development: *"in a context of capital markets imperfections, economic performance, either prosperity or stagnation, depends on the initial wealth distribution"* (Banerjee and Newman, 1993). I sketch a stylized model for labor force participation that includes heterogeneous fixed costs to enter the labor force and frictions in credit markets[5]. In this environment, there is selection into employment based on the initial household wealth; fixed costs generate the need for funds and credit market frictions create difficulties in getting these funds. As a result, households have lower consumption levels because they can't work and they can't work because they are too poor–a poverty trap. The main empirical implication of this model is that an income shock can push people into the labor force, at least for agents who are close to being able to cover their fixed costs.

[2]The effects on work hours represent 6% of the baseline mean, conditional on working.

[3]These results also complement existing evidence regarding the role of the condition component (Baird et al. (2011), Benhassine et al. (2015),Filmer and Schady (2011),de Brauw and Hoddinott (2011)).

[4]In Bolivia, for every 10 male household heads who work, there are only 7 female household heads or spouses of household heads working. This gap has remained constant over the last decade, according to data from Bolivian household surveys.

[5]I allow these frictions to arise either due to high intermediation costs that result in higher borrowing rates, or simply through constraints in the maximum amount each household can borrow.

To test the importance of these two features of the process of development, I derive two additional predictions from the model and take them to the data. First, because of the fixed costs to enter the labor force, the effects of the income shock should affect the decision to enter the labor force but should not affect the intensive margin of labor supply. Estimating treatment effects along the cumulative density function of weekly work hours, I find that the effects on labor supply come from the extensive margin rather than the intensive margin, supporting the fixed-costs hypothesis. Moreover, if the fixed costs are salient, then the results should come from activities that require a fixed cost to work. I find that the effect on employment comes from people shifting from unemployment to self-employment. Second, the effects of an income shock should be stronger when capital market frictions are more salient. Using baseline data for the supply of financial services at the municipality level as a third difference, and controlling for potential treatment effect heterogeneity across urban and rural areas, I find that the effects on labor supply are higher for women from more credit-constrained areas.

Why would agents respond to positive income shocks by increasing labor supply? The evidence provided in this study shows that two particular features of the process of development–fixed costs to work and imperfections in credit markets–explain why the labor supply responds differently to income shocks in developed countries than in developing countries: In developed countries, capital market frictions and labor market frictions are smaller and households behave as in the neoclassical model. In developing countries, households live in an environment of liquidity constraints and fixed costs, and this explains why dependence on cash transfers might be less likely. Moreover, if the process of development is about occupational choice, the evidence in this study suggests that escaping involuntary unemployment is the first step in this process.

To understand the extent to which other mechanisms could lead to the same empirical results, I compare the empirical evidence and the implications of the theoretical framework with compelling alternative explanations in this context. In particular, I discuss two relevant mechanisms: relaxed time constraints and aggregate demand changes due to the program. First, I analyze whether increases in labor supply may be driven by a relief in the mother's time constraints by the program. Since the program provides resources conditional on attendance at school, it might be the case that the observed treatment effects arise because mothers simply reallocate time from child care to productive activities. Three facts rule out that mechanism. First, the program was implemented in a baseline context of high attendance and enrollment and low dropout rates [6]. Moreover, this mechanism would imply that the treatment effects should come from adults whose children are more likely to be affected by the condition component (marginal children). I find that the responses in labor supply are driven primarily by mothers of children who would have attended school even in the absence of the program. Finally, consistent with the latter facts, I find no evidence of effects of the program on enrollment nor on child labor.

Another possible explanation for the positive effects on labor supply could arise from shifts in aggregate demand at the local (municipality) level [7]. Although the transfers were small, the program reached a large share of the households with school-age children. This cash inflow could modify the business environment, favoring self-employment, or it could increase wages, thereby inducing households to work. Two arguments rule out this potential mechanism. First, if local demand were driving the results, the treatment effects should be a function of the ability of households to take advantage of the new context, and therefore the treatment effects should be higher for households in areas that have better access to credit; I find the opposite. Second, in this study, treatment effects are identified from individual variation within clusters (municipalities) since entry into treatment is orthogonal to locations and is a function of individual characteristics only (years of schooling for children of school age). This design provides a way of replicating the natural experiment in each cluster; if either the economic conditions changed or wages increased, they did so similarly for treatment and control groups.

[6] Reports from the Ministry of Education (Zambrana et al., 2004) show that before the program was implemented, the national attendance rate was above 80%, enrollment was above 90% and the dropout rate was below 10%.

[7] One particular challenge in interpreting the reduced form treatment effects from large-scale cash transfer programs is the presence of general equilibrium effects that are confounded with direct income shocks on treatment units (Acemoglu, 2010). Studies such as (Kaboski and Townsend, 2012) and (Muralidharan et al., 2017) analyzing large-scale programs that imply a significant injection of liquidity into the local economy find evidence of general equilibrium effects manifested through wages, in the case of micro-credit in Thailand and a reduction in leakage of resources from a workfare program in India, respectively.

This paper reconciles evidence regarding labor supply responses to income shocks from developed and developing countries using a basic idea: the interaction of frictions in labor and credit markets, which is core to development economics (Banerjee and Newman, 1993) (Lewis, 1954) (Gollin, 2014). The evidence provided by this study contributes to four strands of the literature. First, it provides novel evidence regarding positive income-elasticities of work outcomes, suggesting that a trade-off between short-run poverty alleviation and dependence may not be salient in developing economies (Banerjee et al. (2015),Skoufias and Maro (2008),de Brauw et al. (2015), Alzua et al. (2013) and Fiszbein et al. (2009)), suggesting a win-win scenario for long-run poverty alleviation consistent with Gertler et al. (2012). In particular this paper complements evidence and key insights from Blattman et al. (2014) who also find positive effects on work hours after a randomly assigned cash grant to groups of young entrepreneurs in Uganda. Although the fixed-costs and credit-constraints hypotheses are discussed in that paper, because the study focuses on a particular sample of credit-constrained beneficiaries there is little variation in the sample to test empirically for treatment-effect heterogeneity in that dimension. In this paper I exploit a large-scale nationwide program that captures enough regional variation in credit market imperfections.

Second, the theoretical framework proposed and tested in this paper and the design of the program provide insights for understanding why other studies were not able to find positive responses. Successful, emblematic CCT programs are means-tested and therefore affect a particularly disadvantaged share of the population. If the households that can take advantage of the income shock are only those who are close to covering their fixed costs, as the theoretical framework suggests, programs that focus exclusively on the most disadvantaged agents will fail to capture agents who would potentially use the extra resources as a tool to escape involuntary unemployment. The Bolivian program studied in this paper is not means-tested and reaches around 90% of school-age children, capturing the entire distribution of fixed costs and credit constraints and providing the empirical approach with enough power to capture positive responses in labor supply due to the program.

Third, through having an empirical design that minimizes the role of changes in local demand as drivers of the effects on labor supply[8], this paper focuses on household-level shocks and is related to recent literature providing evidence of the salience of micro-level rather than macro poverty traps(Kraay and McKenzie, 2014). Recent literature regarding low-cost interventions aiming to break these vicious circles focuses on the role of reducing fixed costs that generate low uptake of profitable investments (Bryan et al. (2014),de Mel et al. (2008), Field (2007) and Cascio (2009)). This paper identifies a complementary comparative statics exercise; instead of reducing fixed costs, it modifies non-labor wealth, reducing the salience of these fixed costs. The evidence suggests that when fixed costs are heterogeneous and hard for policy makers to identify, as is most likely the case for large-scale anti-poverty programs, cash transfers are a powerful option that complements other interventions aimed at reducing fixed costs.

Finally, the results complement evidence regarding the importance of credit constraints and capital markets in developing countries. Interventions that attempt to expand credit markets have delivered modest results overall Banerjee et al. (2015), however the results of this program suggest that these interventions can be effectively complemented by small grants in areas that are subject to high intermediation costs that result in credit market frictions.

2.2 The setting

The Bono Juancito Pinto (BJP) program was first announced in October 2006. The program provided a cash transfer (CCT) of 200 bolivianos (approximately 25 U.S. dollars) conditional on 80% school attendance for every child enrolled in public school.[9] As opposed to most programs in the region, this program was not means-tested and the eligibility criterion was based on the grade the child was enrolled in, regardless of their socioeconomic status. This

[8]The design of the program is different from previous experimental evidence from emblematic programs in which random assignment of CCTs is conducted at the cluster level, such as Shultz (2004) in the case of PROGRESA or the studies reviewed by Banerjee et al. (2015), and contributes to the literature with elasticities coming exclusively from increases in the budget sets.

[9]In the baseline year, this accounts for 90% of children enrolled in either private or public schools.

transfer represents around one-third of the monthly minimum wage for the baseline year, 4% of average per capita yearly consumption, around 53% of the yearly per capita education spending in urban areas and more than 100% in rural areas.[10] As of 2005, the school enrollment rate was already high, at 90%. Moreover, dropout and non-passing rates were below 10% before the program was implemented[11]

In the first stage, the potential beneficiaries were children enrolled in first to fifth grades; children who met the attendance threshold and fulfilled additional documentation requirements received the transfer at the end of the school year (November).[12] The resources were disbursed by personnel from the Armed Forces in each school, leaving very little room for leakage or implementation failures.[13] In October 2007, the program was extended to children in sixth grade, again with disbursement of the funds at the end of the school year. The set of beneficiaries was expanded to children in seventh and eighth grades in July 2008, but the disbursement schedule was changed to two payments in July and November 2008. Although the funds were disbursed in two payments, the total amount given to each student did not change.

The program was implemented in a context in which employment for adult females is significantly lower than that of males. Figure 2.1 shows that for male household heads or male spouses of household heads, the share of individuals that report having worked, performed remunerated activities or tasks for a family business during the week preceding the data collection date is around 95%. On the other hand, the ratio is around 70% for female household heads or female spouses of household heads. Two main lessons are suggested by these results. First, the high employment rates for males suggest that job opportunities exists in this economy. Second, despite job availability, the broad gender gap in employment suggests that women face constraints to entering the labor force. This feature of the Bolivian economy motivates the main

[10]Source: Own calculations based on Household Surveys (2005-06) from the National Bureau of Statistics (INE).

[11]Source: Ministry of Education, see Zambrana et al. (2004).

[12]A birth certificate or ID were required; in addition, children had to be accompanied by a parent or guardian to receive the money, generally the mother. After the second round of the program, children who did not possess a birth certificate or an ID could receive the money if they presented two witnesses who testified to their identity.

[13]Self-reported data from national household surveys show that 90% of eligible children report receiving the transfer during the first year of implementation

question of this paper: Can income shocks mitigate some constraints agents face when deciding whether to work?

2.3 Data

The data for this study come from national household surveys conducted by the National Bureau of Statistics (INE) for the years 2002-2009. I constructed a pooled cross-section data set based on 8 waves of household surveys. These surveys are independent cross-section samples of individuals drawn from a common sample frame based on the 2001 population census. Surveys for the years 2002 and 2005 to 2009 were conducted between late November and December of each year. The 2003-2004 survey is a continuous survey applied to different households in two rounds: November 2003-April 2004 (2003 round) and May-October 2004 (2004 round).

In this study, I use a sample of children between 7 and 17 years old who have completed at most eighth grade and who do not report being enrolled in a private school; the sample accounts for 90% of the children of school age. For each child, I compute information regarding the adults living in each child's household and labor market variables for the head of household and the head of household's spouse. I focus on household heads and heads' spouses as, on average, they represent most of each household's income. I use two main measures of employment: The first is an indicator of whether the interviewee reports having worked or performed remunerated activities or tasks for a family business during the week preceding the survey. The second measure refers to the average hours worked per week. To construct this measure I use self-reported information regarding the average number of hours worked per day and the number of days worked in the week preceding the interview. In the case of unemployed people, the number of hours is 0. I focus on these two measures as they are the standard measures used in studies analyzing responses of labor supply to cash transfers in developing countries such as (Alzua et al. (2013) and Banerjee et al. (2015)).

I complement this dataset with information regarding the number of branches of financial institutions and population at the municipality level. Information regarding the number of branches of financial institutions comes from the national regulator, the Authority of Supervision for Financial Institutions (ASFI), and only covers municipalities that are also provincial capitals (112 of 339 municipalities), which account for two-thirds of the observations in my sample. Population data come from the 2001 National Population Census conducted by INE. Summary statistics for 2005, the year preceding the implementation of the program, are presented in Table 3.1.

2.4 Identification strategy

I take advantage of the design of the BJP program to estimate its causal effects on adult employment. I use the staggered timing and eligibility criteria of the program as the identifying sources of variation. Although the program was implemented in all regions of the country at the same time, children were included as beneficiaries of the program gradually, based on years of schooling. Thus, the design provides variation over time and across individuals in a given year, suggesting a difference-in-difference approach. The program's design is presented in Table 2.1.

In order to identify the causal effects of the program on work measures, I use the timing of the program's announcements, which is arguably exogenous to households' decisions, as a first source of variation. The program includes the entire public-school system. Recall that the program was first announced in October 2006, while two expansions were announced later, in October 2007 and July 2008. These dynamics are represented in the columns of Table 2.1 [14]. Cash was disbursed at the end of the 2006 and 2007 school years (November) and in two payments in July and November of 2008.

Second, the design of the program provides cross-sectional variation at each year based on the program's eligibility criterion. This variation is summarized in the columns from Table 2.1. In the first round of the program, children from first to fifth grade were eligible (children

[14] The program was first announced in the first year of the administration elected in November 2005, which suggests that the announcement was unexpected with respect to the set of information the population had in 2005.

Table 2.1. Program Design

Years of Schooling	2002-2005	2006	2007	≥2008
1	C	T	T	T
2	C	T	T	T
3	C	T	T	T
4	**C**	**T**	**T**	**T**
5	**C**	**T**	**T**	**T**
6	**C**	**C**	**T**	**T**
7	**C**	**C**	**C**	**T**
8	**C**	**C**	**C**	**T**
>8	C	C	C	C

Note: Columns report the year in which the information was collected. Rows report the grades in which children can be enrolled. The entries in the table represent the treatment status of each group at each moment in time. "C" denotes groups that belong to the control group in a particular year, that is, groups that are not beneficiaries of the program at that moment in time. "T" denotes groups that belong to the treatment group in a given year; that is, children who, given their years of schooling (grades completed) are treated or not in a particular year. Bold letters denote the groups that are included in the main analysis in this study.

with 1 to 5 years of schooling in the sample[15]), thus they constitute the treatment group for the first round (2006). The control group are children from sixth to eighth grade (6 to 8 years of schooling). Due to the program's expansion, in the second round children in sixth grade enter the treatment group and in the third round, children in seventh and eighth grades are added to the treatment group. These variations suggest a difference-in-difference approach that compares changes over time in the employment rates for parents of children in the treatment group before and after the program with changes over time in the employment rates for parents of children in the control group before and after the program.

In Section 2.8, I discuss two potential problems with my empirical approach. First, as younger children are more likely to induce different opportunity costs for parents' time than older children, I restrict the sample used in the main analysis to children between fourth and eighth grade. This sample selection is represented in bold letters in the entries in Table 2.1. Results using the whole sample (first-eighth grade) are presented in Appendix Table 9; these results

[15]For this study, preschool is not considered in the computation of years of schooling.

do not differ from the main results of the paper. Second, the units of observation are children as treatment assignment is at that level. However, note that among the sample of students, it is possible that some treatment children have siblings in the control group; this implies a 40% reduction of the sample and therefore a loss of statistical power. I present the results for the entire sample, acknowledging that my estimates are likely to represent a lower bound. In Appendix Table B.3 I indeed show that the estimates are higher but noisier once I exclude children whose siblings are in a different treatment group.

2.5 Labor supply responses and the CCT program

In this section I provide evidence of positive treatment effects of the program on female employment through an event-study approach and a difference-in-difference approach. I interpret the result from both approaches as reduced form effects (intention-to-treat effects). Figure 2.2 shows cash reception rates after the program announcement. Compliance is high in this context for all the policy years.

Figures 2.3 and 2.4 show that there was an increase in the total number of hours/week dedicated to work by adults right after their children entered the treatment group. A similar pattern is observed for the total number of adults who report working during the week before the interview. Figure 2.5 shows that the hours dedicated to work and the proportion of adult females (heads of household or spouses) who report working during the week preceding the interview jump abruptly during the first period in which their children enter the treatment group. Work outcomes for adult males (heads of household or spouses) exhibit a smooth trajectory over time. These results suggest that there were increases in work outcomes for adult females as a consequence of the program. To test this hypothesis more rigorously, I estimate a flexible difference-in-difference model using the following specification:

$$Y_{ismt} = \alpha_0 + \mu_m + \delta_t + \theta_s + \sum_{j=-6}^{j=-2} \beta_j 1[\tau_{st} = j] + \sum_{k=0}^{k=4} \beta_k 1[\tau_{st} = k] + \varepsilon_{ismt} \tag{2.1}$$

Y_{ismt} represents the work outcome of interest for the head of household or head's spouse from child i's household. θ_s denotes child i's years of schooling fixed effects, μ_m denotes municipality fixed effects and δ_t denotes year fixed effects. Time to treatment is denoted by τ_{st}. The omitted category is $\tau_{st} = -1$ which denotes the year before a child with s years of schooling enters the treatment group. Standard errors are clustered at the municipality level.

Figures 2.6 and 2.7 plot the point estimates for β_j and their respective confidence intervals for work outcomes. Again, Figure 2.7 shows that there is a significant jump in the hours/week worked and employment status for adult females.

To assess the validity of the common trends assumption, I test two null hypotheses. First, I test whether the sum of the difference-in-difference coefficients β_j for the periods preceding the program is different than zero. Panel A in Table 2.3 shows that it is not possible to reject the null hypothesis of $\beta_{-6} + ... + \beta_{-2} = 0$ for all the work outcomes. Complementarily, Panel B in Table 2.3 shows that it is not possible to reject the null hypothesis that all the difference-in-difference coefficients for periods preceding the program are jointly zero ($\beta_{-6} = ... = \beta_{-2} = 0$).

To capture the average impact of the program for all the periods following the intervention and to increase statistical power, I estimate treatment effects following a standard difference-in-difference approach:

$$Y_{ismt} = \alpha_0 + \mu_m + \delta_t + \theta_s + \beta T_{st} + X_{ismt}\gamma + \varepsilon_{ismt} \tag{2.2}$$

Again Y_{ismt} denotes the outcome of interest. T_{st} is an indicator that takes the value of 1 for the periods in which children with s years of schooling enter the treatment group (i.e.$\tau_{st} \geq 0$).

Table 2.4 presents treatment effects for working outcomes; the results are robust even after including group-specific linear time trends. Considering suggestive evidence of differential shocks between the treatment and control group in period $\tau_{st} = -5$, depicted in Figures 2.6 and 2.7), it is important to know that if anything these pre-trends bias the estimates towards zero.

Two results are worth considering: first, there is no evidence of negative effects on work outcomes. In most specifications it is possible to reject the null of negative treatment effects $\beta < 0$ at 10%. In the case of work outcomes for adult males, the point estimates are precisely estimated zeros. These results confirm evidence from previous studies of CCT programs (Alzua et al. (2013), Banerjee et al. (2015) and Skoufias and Maro (2008)).

More importantly, there is evidence of positive treatment effects for females both at the extensive and intensive margins. I find that the program increases the number of hours/week that female household heads report by 2.5 units and it induces an increase of 4 percentage points in the probability of being employed for female heads. These effects represent 9% of the baseline mean in the case of work hours (6% conditional on working) and 6% in the case of employment. The effects are small, and consistent with a small income shock induced by the CCT program. These results are also consistent with previous evidence found by Alzua et al. (2013) and Skoufias and Maro (2008) in the context of the *PROGRESA* program in Mexico for work hours for females. The results also complement suggestive evidence of positive effects on employment from the *Bolsa Familia* program in Brazil (de Brauw et al., 2015)[16]. Consistent with a context in which there is a large, stagnant gender gap in employment, the positive effect of the cash transfer program manifests in the most disadvantaged population: adult females who are household heads or heads' spouses.

2.5.1 Cash or condition?

To have a better understanding of the nature of the shock and analyze the extent to which the increase in labor supply was driven by either the cash or condition component of the program, I test for heterogeneity in treatment effects based on how binding the condition component of the program was. Understanding which feature of the program induced the treatment effects observed in the previous section will shed light on the interpretation of the program as either an

[16]de Brauw et al. (2015) use a propensity score re-weighting approach that relies on selection on observable characteristics. The empirical approach in this study contributes with novel evidence from a natural experiment.

income shock (cash) or a relief of adult females' time constraint (condition). Evidence regarding the role of condition in CCT programs is mixed: de Brauw and Hoddinott (2011) and Filmer and Schady (2011) provide evidence of a stronger role of the condition component of these programs. Yet Baird et al. (2011) show that even an unconditional cash transfer (UCT) can induce changes in behavior in the direction intended by the condition component of CCT programs; Benhassine et al. (2015) show that simply labeling a UCT as a CCT is enough to encourage the intended behavior. In order to provide a deeper understanding of the type of shock induced by the program, I test whether the impact comes from parents of children for whom the condition component was binding or from parents of children who didn't modify behavior in order to receive the transfer.

The condition component of the program required that children attend 80% of school days during the school year in order to receive the transfer. To test whether the treatment effect comes from marginal or inframarginal agents, it would be ideal to compute treatment effects for children whose baseline attendance rate is below 80% (marginal agents) and for those whose baseline attendance rate is above 80%. Since the dataset in this study does not follow children over time, I do not observe the attendance rate in the absence of the program or at baseline. Nevertheless, I use the 2004 round of the household surveys, a baseline year, to estimate a probit model for attendance using demographic characteristics. I then use the coefficients to predict the 2004 attendance rate for all the children in the study sample [17].I interpret this predicted attendance rate as the average attendance rate a child would have, had the child been observed in the 2004 sample; this is a counterfactual baseline attendance rate.

The 2004 round of the survey is particularly useful for two reasons. First, the information was collected during the months of May to November of 2004, covering most of the school year. In other years, the household survey data was collected in December, once the school year had ended. Since the period of reference in the surveys is the week before the survey interview,

[17] The probit model was estimated using a full set of dummy variables regarding age and years of schooling; household indicators, including indicators for whether the household is located in a rural or urban area, the number of people in the household, and whether the head of household is male; children's characteristics such as gender; and indicators for speaking Spanish as a first language and whether the survey respondent self-identified as indigenous. Appendix Figure B.4 shows that the model has good out-of-sample prediction power across all the age categories.

most interviewees respond that children didn't attend school because of summer vacation.[18]. However, this is not the case in the 2004 wave as it covers a period that coincides with the school year. Second, the 2004 wave provides information regarding school attendance based on several months rather than just a single month as opposed to the rest of the surveys. As the sample is random, for each child interviewed in month m of the 2004 wave, there is another similar child interviewed in the upcoming months; this means that this attendance rate also captures variation across months within the school year. Figure 2.8 depicts the distribution of the baseline counterfactual attendance rate. Note that around 80% of the sample corresponds to children with an attendance rate above the condition.

Table 2.6 reports triple differences estimates using the predicted baseline attendance rate as a third difference (columns (1) and (4)). I interpret this third difference as a measure of the salience of the condition component in the program. For children with a low baseline attendance rate, the shock induced by the program is interpreted as a mix between cash and condition; for children with a high baseline attendance rate, the shock induced by the program is interpreted as a pure income shock as these children would have attended school in the absence of the program. The results suggest that the treatment effects on work outcomes are an increasing function of the baseline attendance rate. The treatment effects evaluated at the 90th percentile of attendance rate are 3.4 hours (p-value=0.002) and 0.05 percentage points (p-value=0.001) for hours and the probability of work respectively. Estimates at the 10th percentile are very small and statistically not different from zero in both cases (see bottom panel of Table 2.6).

To test this hypothesis with higher power, I estimate a triple-difference model using an indicator of whether child i's attendance rate is below the condition threshold (0.8, see columns (2) and (5)) and whether child i's attendance rate is below the median (columns (3) and (6)). Results show that work outcomes for adults related to inframarginal children are higher. The effects for marginal children are even null in the case of hours/week and not significant in the case of employment. In general, the positive impact on employment for adult females related to

[18]For those children not on vacation, the average attendance rate is 98%.

inframarginal children accounts for 90% of the overall treatment effect computed in Table 2.4. This result is not surprising as schooling outcomes were already high before the program was implemented. Moreover, the announcements of the implementation and expansion of the program were made once the school year was close to its end; for example, the first announcement was made in October 2006, a month before the school year was over, leaving reduced scope for behavior adjustment in order to meet the conditions. Appendix Table B.4 shows that there were no overall effects on employment for children and small effects on enrollment that vanish once I allow for group-specific trends. All together, the evidence suggests that the effects of the program on labor supply come mostly from an income shock.

2.6 Dependence or constraints?

The results from the preceding sections contradict evidence from developed countries showing small negative responses in labor supply after exposure to cash welfare programs (Hoynes (1996) and Hoynes and Schanzenbach (2012)) and are consistent with evidence from developing countries that fails to find negative effects of cash transfer on adults' labor supply (Alzua et al. (2013),Banerjee et al. (2015)). In this section, I outline a simple framework that unifies these divergent results. I do so by referring to a traditional idea behind the process of development: In a context of imperfections in capital markets, economic performance (either prosperity or stagnation) depends on the initial wealth distribution (Banerjee and Newman, 1993). I sketch a stylized model for labor force participation that includes fixed costs to enter the labor force and frictions in capital markets. In this environment, there is selection into employment based on initial wealth. The model suggests three testable implications: *i)* an income shock can push people into the labor force, consistent with the evidence presented in the previous section; *ii)* the effects of an income shock should be bigger when capital market frictions are more salient; and, *iii)* the effects of an income shock should affect the decision to work and not the intensive margin of labor supply.

Consider a household composed of one individual deriving utility u from consumption c_i. For simplicity, let the utility function be $u(c) = c_i$. The household is endowed with initial wealth v_i and allocates hours of labor inelastically to the only possible job always available in this economy, receiving earnings equal to w. There is a cost p_i of entering the labor force. The timing is as follows: In period $t = 0$ the household decides whether to cover the fixed cost using its initial wealth v_i or borrowing a_i, using funds available in complete financial markets at a zero real interest rate. In period $t = 1$, conditional on its decision in period $t = 0$, the household maximizes utility subject to its budget constraint. I assume that if a household decides to cover the fixed cost, the household finds a job instantaneously. For example, this can be the case of self-employment. Let $\lambda_i \in \{0, 1\}$ denote the decision of investing in the fixed cost. If the household decides to invest, then $\lambda_i = 1$; if the household doesn't invest then $\lambda_i = 0$; in the latter case, the household member stays outside the labor force.

This framework is consistent with several fixed costs or frictions discussed in the development economics literature and tries to capture heterogeneity in fixed costs over households. In some cases p_i can be the market value of the minimum caloric intake necessary to conduct a task and be chosen by employers as in Dasgupta and Ray (1986). Alternatively, p_i could represent the cost of attaining the minimum consumption of comfort goods that are necessary for women to focus on working rather than exclusively on household chores as in Banerjee and Mullainathan (2008). Alternatively, p_i could represent the cost of sending children to preschool and therefore free up time to be allocated to labor (Cascio, 2009). In contexts of high salience of self-employment, p_i could represent the value of capital necessary for agriculture or a family business as in de Mel et al. (2008) or Blattman et al. (2014). Fixed costs can also be present outside self-employment; p_i could represent the price of a bus ticket in the context of seasonal migration (Bryan et al., 2014). Fixed costs can be nonpecuniary: p_i could represent the cost of paperwork to obtain land/house titles, as the absence of title could result in lack of labor force participation (Field, 2007).

The household maximizes:

$$\max_{c,a,\lambda} u(c_i) = c_i$$

s. t.

$$c_i = w - a_i \qquad \text{if } \lambda = 1 \text{ and } t = 1$$

$$c_i = v_i \qquad \text{if } \lambda = 0 \text{ and } t = 1$$

$$v_i + a_i = p_i \qquad \text{if } t = 0$$

$$c_i \geq 0$$

Using backward induction, the household will decide to invest in the fixed cost and therefore work if and only if $w \geq p_i$. In this setting, even with frictions in the labor market, working decisions do not depend on initial wealth. Note, however, that with heterogeneity in fixed costs, households that face higher fixed costs will only work if wages are high enough to make it profitable. For instance, in an economy with higher fixed costs for females, there would be a higher employment rate for males at the same market wage. This is consistent with the Bolivian gender gap in employment as discussed in Section 2.2 (see Figure 2.1).

Consider now an environment in which there are intermediation costs for the lender that lead to a risk premium over the interest rate that the household head would earn when depositing her money in a savings account or investing in a risk-free asset. Denote this premium as r. Note that now the household can either decide to self-finance the fixed cost and invest the remaining funds in a zero-real-interest-rate, riskless asset or borrow some money from either a bank or an informal lender at rate $r > 0$. There are no exogenous credit constraints in this economy but there is a spread between lending and saving interest rates that reflects potential frictions in the credit market. In period $t = 0$ the household faces the same budget constraint but depending on whether $a_i > 0$ or not, the household member will face different budget constraints in period $t = 1$.

$$
\begin{aligned}
&c_i = w - a_i(1+r) && \text{if } \lambda_i = 1 \text{ , } t = 1 \text{ and } a_i > 0 \\
&c_i = w - (v_i - p_i) && \text{if } \lambda_i = 1 \text{ , } t = 1 \text{ and } a_i \leq 0 \\
&c_i = v_i && \text{if } \lambda_i = 0 \text{ and } t = 1 \\
&v_i + a = p_i && \text{if } t = 0
\end{aligned}
$$

Suppose household i faces fixed costs $p_i < w$ and is endowed with an initial wealth $v_i \geq p_i$. As borrowing and self-financing are perfect substitutes, this household picks the least expensive option: self-financing. On the other hand, if $v_i < p$ the household can only cover the fixed cost by borrowing at rate r in an amount equal to $a_i = p_i - v_i$. Consider now a household with a high initial wealth v_H such that $v_H \geq \tilde{p}$. This household enters the labor force if and only if $w \geq \tilde{p}$. Thus, this household lives in a context where financial market frictions are not salient. However, the story is different for a household facing the same wages (w) and fixed costs ($\tilde{p}$) but with low initial wealth v_L such that $v_L < \tilde{p}$. In order to work, this household has to finance the fixed cost by borrowing at a rate r. This means that this household will only work if $w \geq \tilde{p} + (\tilde{p} - v_L)r$.

Let $\bar{w}_H = \tilde{p}$ and $\bar{w}_L = \tilde{p} + (\tilde{p} - v_L)r$ denote the reservation wage corresponding to households with high and low income, respectively. Since $\tilde{p} - v_L > 0$, we have that $\bar{w}_L > \bar{w}_H$. This means that households with lower wealth need a higher market wage in order to decide to work. This difference arises because of the interaction of frictions in the labor market (fixed costs p) and frictions in the financial market $r > 0$. In this case poor households have low consumption levels because they can't work, and they can't work because they are simply too poor. Minimal assumptions were needed to generate the possibility of a poverty trap: as in Banerjee and Newman (1993), economic performance, either prosperity or stagnation, depends on where in the distribution of initial wealth a household is located. In this environment, there are three testable predictions from the model.

Prediction 1: A positive income shock can increase the probability of working. Consider a shock ε_i such that $\varepsilon_i \geq p_i - v_L$. This income shock pushes the new income $v_i' = v_L + \varepsilon_i$ above the fixed cost. In this case, poor agents can self-finance its entrance to the work force and will work as long as the market wage w is greater than the fixed cost. This income shock pushes the household from an equilibrium of involuntary unemployment to one with employment. This prediction is consistent with the results found in Section 2.5: An income shock can push people into the labor force. However, note that this effect has a local nature as only the households for whom the income shock is large enough to cover the gap between their fixed costs and wealth endowments will be pushed into the labor force (individuals at the margin); less fortunate households will face binding constraints even after the shock.

Heterogeneity in wealth and fixed costs could explain some stylized facts in the empirical literature on CCTs. Emblematic CCT programs aim to help the most disadvantaged part of the population. In particular, means-testing or proxy-means-testing mechanisms are popular targeting tools.[19]To the extent that these programs effectively target the least advantaged population (i.e., the ones with higher $p_i - v_i$) it could be the case that studies of the impact of CCT programs on labor supply fail to find effects on employment as, given an income shock, the gap between wealth and fixed costs is simply too large. In this study, eligibility for the program is fairly orthogonal to wealth and fixed costs as its design does not involve a means-tested targeting mechanism; therefore the evaluation captures the entire distribution of $p_i - v_i$.

Prediction 2: The effect of an income shock ε_i should be higher when there are borrowing constraints. Consider the case of a household with non-labor income v_L such that $v_L < p_i$. This household would borrow from the bank or informal lender if $\bar{w}_L = p_i + (p_i - v_L)r$. Let $\bar{a} > 0$ denote the maximum amount a household can get from the informal lender. This

[19]Fiszbein et al. (2009) provide a comprehensive summary of targeting mechanisms for CCT programs. Large-scale programs such as PROGRESA and BOLSA FAMILIA follow this approach.

household solves:

$$\max_{c,a,\lambda} u(c) = c_i$$

s. t.

$$c_i = w - a_i(1+r) \qquad \text{if } \lambda = 1\ ,\ t = 1$$

$$c_i = v_L \qquad \text{if } \lambda = 0 \text{ and } t = 1$$

$$v_L + a_i = p_i \qquad \text{if } t = 0$$

$$a \leq \bar{a}_i$$

$$c \leq 0$$

In the interior solution, when the credit constraint is not binding, this household uses the same decision rule as in the unconstrained case and there is still selection into employment arising from the interaction of fixed costs and other frictions in capital markets. Moreover, when credit constraints bind, although it is profitable to work, the household member won't be able to work because of her inability to cover the fixed cost. In a context of credit constraints the problem households face is even more complicated: Even if r is small, households that would like to borrow at the current rate wouldn't be able to borrow optimally; those households facing a credit constraint $\bar{a}_i$ such that $v_L + \bar{a}_i < p_i$ will not work. However, note that an income shock ε_i such that $v_L + \varepsilon_i + \bar{a}_i = p_i$ will push households into the labor force. In this model, both types of financial frictions interact with labor market frictions and yield a result in which households sort into the labor force based on their initial wealth. The increase in the effect of an income shock comes from households who find it profitable to borrow at rate r but can't borrow as much as they would like.

Prediction 3: Income shocks should affect labor supply positively only at the extensive margin. So far, the model sketched in this section doesn't consider labor supply at the intensive margin. This approach was chosen in order to focus only on corner solutions. Interior solutions in a model with a trade-off between consumption and leisure should behave as in the neoclassical model once the agent decides to work; conditional on working, a household chooses how many hours to work, equalizing marginal rates of substitution between consumption and leisure with the real market wage. In this context, an income shock has non-increasing effects on hours worked. Note, however, that a positive effect on hours worked can be observed in a richer model in which time off work can be productive for household consumption as in Becker (1965); in this case, the positive effect requires that households substitute away from time-intensive goods.

2.6.1 Testing the implications of the model

Labor supply and fixed costs to work

In this section, I test for the salience of fixed costs to enter the labor force. I do this in two steps. First, I show that despite finding effects at both the intensive and extensive margin of work for adult females, the effects come mainly from responses at the extensive margin. Second, I show that these effects are associated with increases in the probability of being self-employed due to the program, suggesting that the responses in employment come from small businesses, a sector that faces small but salient fixed costs.

The theoretical framework sketched in this paper suggests that the impacts of an income shock should be related to the extensive margin of labor supply rather than the intensive margin, as I assume that once the decision to work is taken, the agents behave according to a neoclassical model. So far, the results presented in Table 2.5 show significant impacts on hours/week worked by females. Yet the measure of work hours includes zeros for females who do not work. Although fixed costs are unobserved and heterogeneous, if they are salient they should manifest in the labor supply responses to an income shock only at the bottom of the distribution of work hours. To empirically test this hypothesis, I estimate treatment effects along the cumulative distribution function of work hours.

Let H_i denote the hours worked weekly by child i's mother. Let Y_i^x be an indicator function $Y_i^x = 1[h_i > x]$ denoting whether child i's mother reported working more than x hours the week before the interview ($x \in [0, \bar{h}]$).

$$Y^x_{ismt} = \alpha + \mu_m + \delta_t + \theta_s + \beta(x)T_{st} + X_{ismt}\gamma + e_{ismt} \tag{2.3}$$

The parameter of interest is $\beta(x)$, which represents the difference-in-difference estimate for the ITT effect on the cumulative density function of hours/week worked evaluated at x. If there are fixed costs to enter the labor force, then treatment effects should only manifest through the extensive margin. Formally, this means that $\beta(x)$ is a non-increasing function of x with $\beta(0)$ as intercept. Figure 2.9 plots the estimated coefficients $\hat{\beta}(x)$ from (2.3) against x for the case of adult females. Note that the treatment effects, for most values of x, are significant and constant at $\beta\hat{(}0)$. Although there are some increases around $x = 20$ [20], the biggest jump in the treatment effects comes at the bottom of the distribution of working hours, confirming the fixed-cost hypothesis. This result complements evidence from recent literature that analyzes variation in particularly salient fixed costs to work such as Bryan et al. (2014) and Field (2007). This paper identifies a complementary comparative statics exercise; instead of reducing fixed costs, it modifies non-labor wealth and reduces the salience of these fixed costs.

The fixed-costs to work hypothesis suggests that the positive effects on work outcomes should come from a measure of labor markets deeply related to entrepreneurship. Table 2.5 provides evidence of positive treatment effects of the program on self-employment for adult females (heads of household or head's spouse). These effects are not related with work inside the household. The dependent variable is an indicator function that takes the value of 1 for self-employed females and 0 for unemployed females; it measures the transition from unemployment to self-employment. As the cash transfer relieves liquidity constraints, this finding

[20] These extra increases at $x = 20$ are consistent with a context of under-employment or agents overcoming fixed costs for a second occupation.

complements mild positive results on self-employment and business start-up from interventions expanding the supply of microcredit (Banerjee et al., 2015) (Kaboski and Townsend, 2012). Moreover, previous evidence from Mexico (Gertler et al., 2012) shows that the long-term gains in consumption due to the *OPORTUNIDADES* program can be explained by an increase in productive investment induced by the program. The increases in employment for females mostly related to self-employment complements these long-term results with short-term responses in labor supply. Overall, if the process of development is about a reallocation of resources from subsistence agricultural production to entrepreneurship, moving people from unemployment to self-employment could be the first step in that process.

Labor supply responses and credit markets

To test whether the impact of the program is higher for individuals who are either more credit-constrained or face stronger credit market imperfections, I estimate a triple-difference model that extends the difference-in-difference model from equation (2.2) by including a third source of variation: the number of financial institution branches per 100,000 individuals in each municipality at baseline. These data are only available for municipalities that are provincial capitals (112 out of 339 municipalities), however two-thirds of my sample belong to these localities. I interpret this cross-municipality variation as a shift in credit market imperfections: Areas with low supply of financial services have a limited set of financing options for local households, leading to higher credit constraints; they also exhibit less competition for informal lenders, allowing repayment rates to be potentially higher. Columns (1) and (4) from Table 2.7 report triple-difference estimates for hours/week and the probability of working the week prior to the interview. The results show that the effect is higher for females in areas with high credit-market imperfections.

To show that heterogeneity in treatment effects does not come from the fact that rural areas are more credit-constrained than urban areas, I estimate a model that includes a full set of interactions between rural-urban dummies, years of schooling and years fixed effects: a triple-difference coefficient using urban-rural dummies. Columns (3) and (6) show that even accounting for potential treatment-effect heterogeneity across urban and rural areas, the negative slope with respect to access to financial services remains strong and hence the results are not simply driven by treatment-effect heterogeneity due to geography. The results in this paper suggest that the cash transfers were more salient for households that were more likely to face credit constraints.

2.7 Potential alternative mechanisms

In this section I discuss alternative mechanisms that could explain the positive labor supply responses to the program; I also discuss the plausibility of these channels given the evidence found in the empirical exercises presented in this study. I present two alternative explanations: an aggregate demand mechanism induced by the injection of cash into the local economy and the relaxation of adult females' time constraints due to the condition component of the program.

One particular challenge in interpreting the reduced-form treatment effects from studies that evaluate the impact of large-scale cash transfer programs is the presence of general equilibrium effects that are confounded with direct income shocks on treatment units (Acemoglu, 2010). The Bolivian program, despite providing a small transfer to each beneficiary child, injected money into the local economy in a short period of time. If this transfer increased aggregate demand in the local economy and hence wages, then it could be the case that some agents decided to work at that higher wage. This mechanism has been documented in the development economics literature that analyzes general equilibrium effects after large-scale interventions[21]. However, the nature of the shock studied in this paper differs from the shocks induced by other CCT interventions analyzed by Alzua et al. (2013) and Banerjee et al. (2015): in those studies

[21]Alzua et al. (2013) find positive effects of the *PROGRESA* CCT program on wages for males. Similarly, Kaboski and Townsend (2012) and Muralidharan et al. (2017) find increases on wages after the implementation of the Million Baht Fund program in Thailand and a large-scale public works program (NREG) in Andha Pradesh, India, respectively.

the treatment is randomly assigned across clusters and their estimates are based on cross-cluster comparisons. In this study, the treatment effects are identified using arguably exogenous individual and time series variation within clusters, as both specifications in equations (2.1) and (2.2) include municipality fixed effects. This means that potential effects through prices are isolated as comparisons are performed within clusters. If there was an increase in wages, this increase affected the treatment and control groups similarly. Moreover, if the effects were driven by increases in wages, then households who are less exposed to credit-market imperfections should be better able to respond as they can borrow to cover the fixed cost of working. The evidence found in Section 2.6.1 (see Table 2.7) suggests the opposite, as the treatment effect is a decreasing function of the degree of credit-market imperfections.

Second, since the program's main objective was to increase attendance and enrollment among the children who were the beneficiaries, the increase in labor supply for adult females could be explained by the relief of a time constraint rather than an income shock. Two pieces of evidence from this study suggest that this may not be the case. First, the positive treatment effects are driven by beneficiaries who would have attended school even in the absence of the program as discussed in Section 2.5.1. Second, after controlling for differential trends, I can't find evidence supporting increases in enrollment due to the program. Appendix Table B.4 shows difference-in-difference estimates of the program on the probability of enrolling in school the year after each cohort was exposed and the probability of working the year the transfer was disbursed. The evidence suggests that there were not effects on outcomes for children.

2.8 Robustness checks and methodological issues

In this section I discuss two empirical challenges and conduct two robustness checks that rule out potential threats to my identification strategy and my results. First, the main analysis includes children from fourth to eighth grade only, excluding younger children as they may have differential trends arising from differential opportunity costs for parents' time. In Section B of the appendix, I replicate the main graphical evidence from this study but including younger children (see figures B.1 and B.2). Regression results using the whole sample (first-eighth grade) are presented on Appendix Table B.1. The results are fairly similar in all of the specifications. Note that in this case, I am able to detect significant increases in total household labor supply, measured by the total number of work hours for all adults in the household (See panel A in Appendix Table B.1).

Second, since treatment assignment is at the child level, the units of observation in my dataset are children. However, note that among the sample of students, it is possible that some treatment children have siblings in the control group; this would imply that data for their parents is counted both in the treatment and control group. This could be a source of downward bias of the estimates. Since excluding children with siblings with differential treatment status implies reducing the sample by 40% with the resulting loss of statistical power, I present the results for the entire sample, acknowledging that my estimates are likely to be a lower bound. In Section B.1.1 of the Appendix (Table B.3) I show that the estimates are higher but noisier once I exclude children whose siblings are in a different treatment group.

2.9 Concluding remarks and discussion

This paper analyzes whether positive income shocks can cause increases in labor supply using a large-scale conditional cash transfer program implemented through Bolivia's public schools. Contrary to predictions from the neoclassical model and the evidence from cash welfare programs in developed economies, I find that an income shock can push people into the labor force. In particular, I find that this is so for adult females, either household heads or heads' spouses. This result is consistent with systematic evidence from CCT programs in developing countries of non-negative income labor supply elasticities (Alzua et al. (2013) and Banerjee et al. (2015)). I also find evidence that the positive impact of the program on adult females' labor supply comes from women whose children would have attended school in the absence of the program, suggesting that the cash rather than the condition component of the program explains the effects. This result rules out responses in labor markets due to the relief of time constraints for adults.

To understand the economics behind these results, I provide a simple explanation that unifies the results from developed and developing countries. Once I introduce fixed costs to enter the labor force and credit-market imperfections that lead to either high repayment interest rates or borrowing constraints into a stylized labor-force participation model, selection into employment is based on initial wealth. In this environment, two equilibria are present in the economy: one in which agents are rich enough to self-finance the fixed costs to work and another in which the agents are simply too poor to work–a poverty trap. In this context, an income shock can move agents from an equilibrium with involuntary unemployment to one with employment, consistent with the main result of this paper. I find that the program increased the probability of working by 4 percentage points and the weekly hours worked by 2.5 hours for female household heads. These effects are associated with similar impacts on self-employment, a sector with fixed costs. The effects are small as the income shock is small, and are consistent with the theoretical approach in this paper suggesting that the effects come from agents at the margin.

Why do labor supply studies in developed countries find negative income elasticities, but this is not the case for developing countries? The theoretical framework developed in this study suggests that if agents don't face fixed costs and credit constraints, then their behavior should be consistent with the neoclassical model. This should be the case for countries that are far along in the process of development. However, the reality may be quite different in countries that are further down the ladder in this process. Underdevelopment comes with strong barriers to work and credit markets that are far from perfect. When cash aid reaches agents in this environment, some agents may use that money to cover basic needs, while others will find the extra liquidity needed to begin moving out of poverty. As discussed in the theoretical framework, those agents who are lucky enough to be close to covering their fixed costs will exhibit positive labor supply responses.

Why then have other studies in developing countries not found positive effects of income on labor supply? The theoretical framework proposed and tested in this paper and the design of the program provide insights for understanding why other studies were not able to find positive responses. Successful, emblematic CCT programs are means-tested and therefore affect a particularly disadvantaged share of the population. If the households that can take advantage of the income shock are only those that are close to covering their fixed costs, as the theoretical framework suggests, programs that focus exclusively on the most disadvantaged agents will fail to capture agents who would potentially use the extra resources as a tool to escape involuntary unemployment. Studies such as Alzua et al. (2013) and Banerjee et al. (2015) focus on contexts in which the program beneficiaries are simply too poor to take advantage of the shock. The Bolivian program studied in this paper is not means-tested and reaches around 90% of children of school age, capturing the entire distribution of fixed costs and credit constraints. This provides an empirical approach with enough power to capture positive responses in labor supply due to the program.

Altogether, the results suggests that an apparent trade-off between immediate poverty reductions and long-term poverty alleviation might not be salient in contexts of fixed costs to work and credit constraints, two key features of developing economies. This potential trade-off would arise from dependence generated by these income transfers; nonetheless, the results suggest that constraints rather than dependence may explain vicious circles of poverty. Consistent with recent evidence regarding investments in human capital and skills after winning cash grants (Blattman et al., 2014)and long-term improvements in consumption driven by agricultural investment in Mexico (Gertler et al., 2012), the results suggest that the first step to climbing the ladder of development is overcoming the barriers households face to simply start working.

2.10 Figures

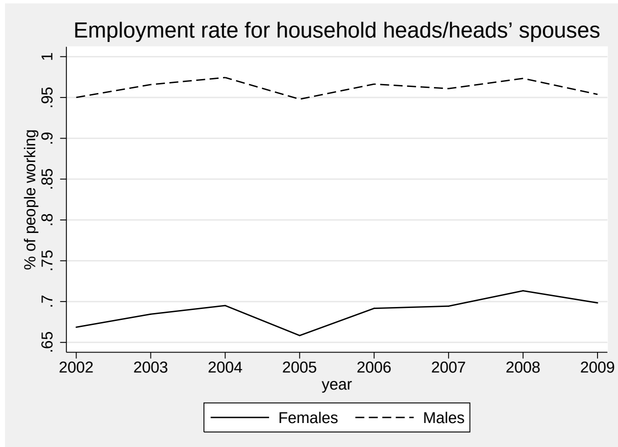


Figure 2.1. Gender Disparities in Employment

The figure depicts employment rates for female and male heads of household or heads' spouses, on the left axis. Employment rate is measured as the share of people of working age who report having worked the week prior to the survey interview.

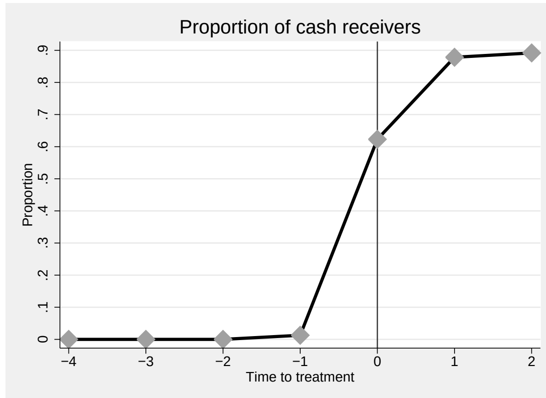


Figure 2.2. Cash Reception

The figure shows the proportion of beneficiary children who report having received the transfer for each year before and after the exposure of child *i* to the program. Time to treatment is equal to 0 in the first period in which treatment kicks in. Uptake rates are computed based on self-reported information regarding the year preceding the survey interview.

Figure 2.3. Total Hours Worked (per week) - Household Adults

The figure depicts means for the total weekly hours worked by adults in child *i*'s household before and after child *i* is exposed to treatment. Time to treatment is equal to 0 in the first period in which treatment kicks in.

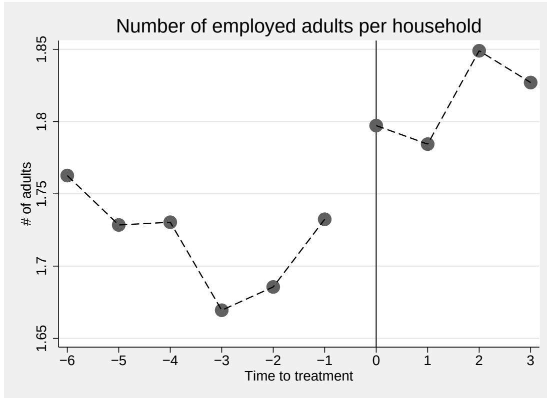


Figure 2.4. Number of Adults Working

The figure depicts means for the number of employed adults in child i's household before and after child i is exposed to treatment. Time to treatment is equal to 0 in the first period in which treatment kicks in.

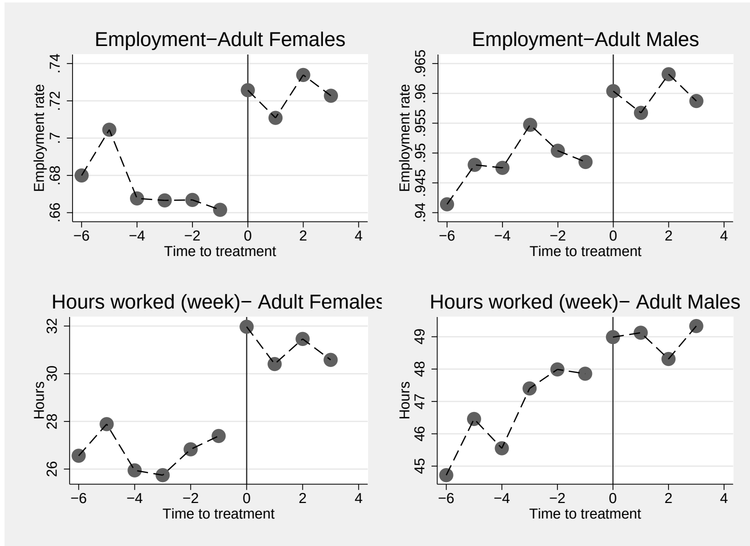


Figure 2.5. Employment and Hours Worked (weekly) for Adults

The top panels depict employment rate for adult males (heads of household or spouses) and adult females (heads of household or spouses) in child *i*'s household before and after child *i* is exposed to treatment. The bottom panel depicts weekly hours for both adult males and females. Time to treatment is equal to 0 in the first period in which treatment kicks in.

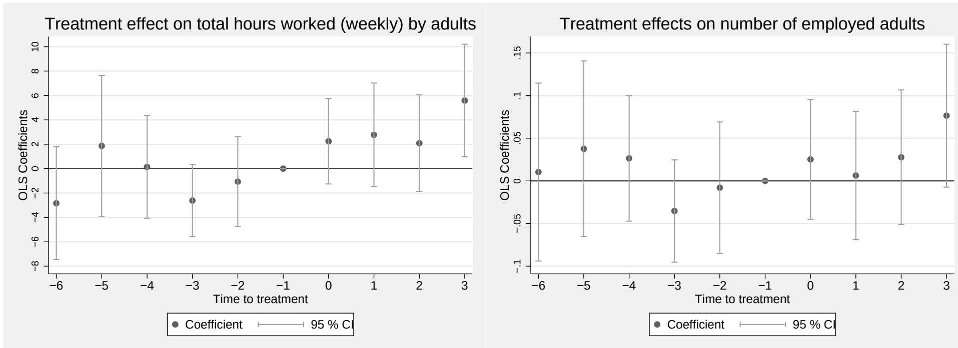


Figure 2.6. Treatment Effects on Total Household Labor Supply (Adults): Total Weekly Hours Worked (left) and Number of Adults Working

The figure depicts OLS coefficients from equation (2.1). Left-hand panel: Ech coefficient estimates differences in differences on hours worked by adults between the treatment and control group with respect to the period just before the program was implemented ($\tau = -1$). The dependent variable measures the total number of hours worked by adults in child i's household. Standard errors are clustered at the municipality level. Right-hand panel: Each coefficient estimates differences in differences on adult employment between the treatment and control group with respect to the period just before the program was implemented ($\tau = -1$). The dependent variable measures the number of adults employed in child i's household. Standard errors are clustered at the municipality level.

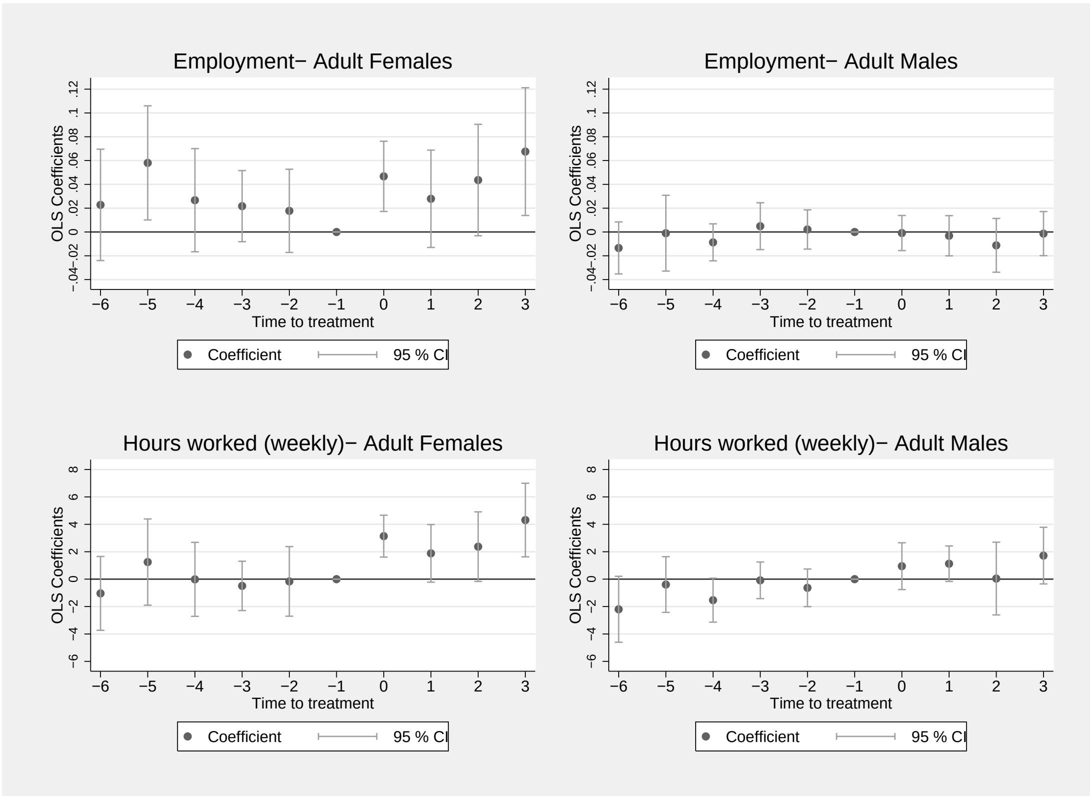


Figure 2.7. Treatment Effects on Employment and Hours for Adults

The figure depicts OLS coefficients from equation (2.1). Each coefficient estimates differences in differences on the relevant measure of labor supply between the treatment and control group with respect to the period just before the program was implemented ($\tau = -1$) . The top panel depicts effects on the probability of working, the bottom panel depicts effects on weekly work hours. The plots on the left are the results for adult males while those on the right are results for adult females. Standard errors are clustered at the municipality level.

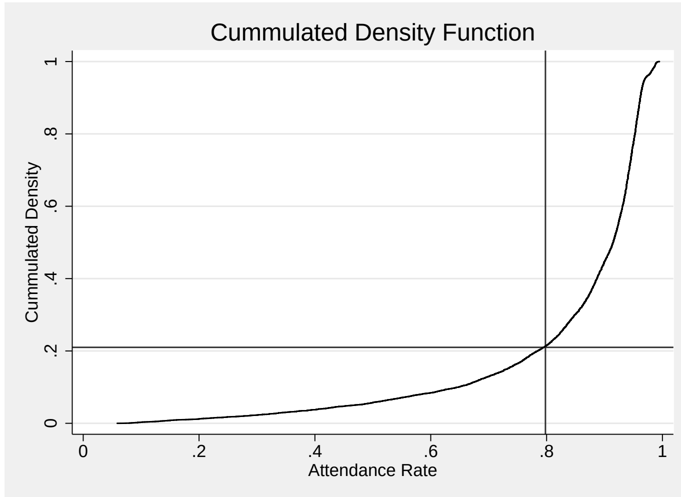


Figure 2.8. CDF of Predicted Attendance Rate

The figure plots the cumulative probability function for the counterfactual attendance rate. The vertical line denotes the cutoff determined by the condition component of the CCT program, while the horizontal line denotes the proportion of the sample located below the condition cutoff.

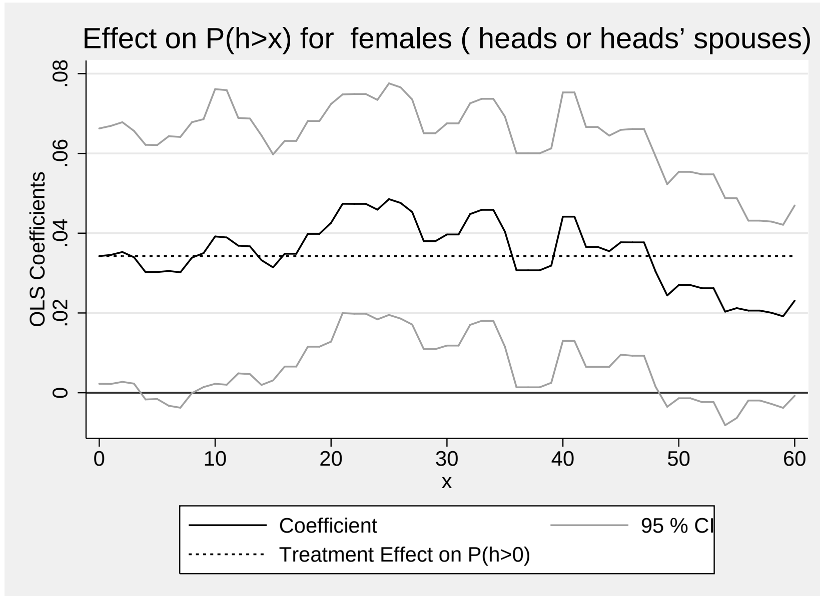


Figure 2.9. Treatment Effects Along the Distribution of Weekly Work Hours for Adult Females

The figure depicts treatment effects estimated through OLS based on (2.3). Each coefficient estimates differences in differences on the probability of working at least *x* hours between adult females belonging to households from treated children and control children, before and after the program. Standard errors are clustered at the municipality level.

2.11 Tables

Table 2.2. Summary Baseline Statistics

	N	Mean	Std.Dev	Min	Max
Panel A: Work Outcomes (Adults - Household)					
Total hours/week	2520	76.51	46.68	0	211
Number of adults who worked last week	2556	1.69	0.89	0	4
Number of self-employed adults	2536	0.69	0.64	0	2
Number of adults working at home	2534	0.05	0.22	0	1
Panel B: Work Outcomes (Female household heads / heads' spouses)					
Total hours/week	2397	26.27	25.54	0	84
Total hours/week (conditional on working)	1566	40.20	20.92	1	84
Worked last week	2417	0.66	0.48	0	1
Self-employed	2417	0.29	0.45	0	1
Works at home	2417	0.05	0.22	0	1
Panel C: Work Outcomes (Male household heads / heads' spouses)					
Total hours/week	2090	47.35	21.41	0	91
Total hours/week (conditional on working)	1977	50.06	18.69	2	91
Worked last week	2119	0.95	0.22	0	1
Self-employed	2119	0.47	0.50	0	1
Works at home	2119	0.02	0.14	0	1
Panel D: Work/Schooling Outcomes (children - 7-18 years old)					
Total hours/week	2560	6.88	14.27	0	60
Total hours/week (conditional on working)	729	24.15	17.28	2	60
Worked last week	2560	0.28	0.45	0	1
Enrolled in school	2560	0.91	0.28	0	1
Panel E: Household Characteristics					
Urban Area	2560	0.51	0.50	0	1
Self-identified as Indigenous	2560	0.63	0.48	0	1
Spanish as first language	2119	0.54	0.50	0	1
Number of household members	2560	5.93	2.12	1	18
Number of adults in household	2560	2.27	1.06	0	9
Number of children under 5 in household	2560	0.62	0.85	0	5

Note: The table presents summary statistics for children with 4 to 8 years of schooling as of 2005, the year preceding the program. Panel A presents statistics regarding aggregate data at the household level for household members older than 18. Panels B and C present statistics for the household head or spouse in the case of adult females and males, respectively. Panel D reports information regarding children between 7 and 18 years old. The variables regarding employment are computed based on indicators of whether or not each person in the household reported working in the week before the interview. Hours worked are computed with self-reported information regarding the average number of working hours per day and the average number of days worked in the week before the interview.

Table 2.3. Testing for Parallel Trends

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Adults		Adult Females -hh heads		Adult Males - hh heads	
	Hours	Works	Hours	Works	Hours	Works
Panel A: $H_0 : \beta_{-6} + \beta_{-5} + ... + \beta_{-2} = 0$						
$\hat{\beta}_{-6} + \hat{\beta}_{-5} + ... + \hat{\beta}_{-2}$	-4.50	0.03	-0.47	0.15	-4.84	-0.02
F-stat	0.27	0.03	0.01	3.02	2.43	0.16
P-val	0.61	0.86	0.93	0.18	0.30	0.69
Panel B: $H_0 : \beta_{-6} = ... = \beta_{-2} = 0$						
F-stat	2.00	1.46	1.26	1.54	1.22	1.05
P-val	0.08	0.20	0.28	0.08	0.12	0.39

Note: The table presents tests for common pre-trends between treatment and control groups based on the flexible difference-in-difference model described in (2.1). Standard errors are clustered at the municipality level. Panel A tests the null hypothesis that the sum of all the coefficients capturing differential trajectories between the control and treatment groups from each year preceding the implementation of the program with respect to the year preceding entrance to treatment. Panel B, tests the null hypothesis that all pre-trend coefficients are jointly equal to zero.

Table 2.4. Effects on Employment

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Work Outcomes (Adults - Household)						
	Total hours/week			Total working adults		
TE (DD)	1.938	1.950	2.200	0.019	0.013	0.012
	(1.732)	(1.533)	(1.638)	(0.036)	(0.030)	(0.032)
Observations	18,194	17,434	17,434	18,309	17,543	17,543
R-squared	0.008	0.160	0.161	0.006	0.250	0.250
Mean DV	79.37	79.37	79.37	1.732	1.732	1.732
Panel B: Work Outcomes (Female household heads / heads' spouses)						
	Hours/week			Worked last week		
TE (DD)	2.591***	2.507***	2.336***	0.039***	0.039***	0.034**
	(0.751)	(0.715)	(0.804)	(0.014)	(0.014)	(0.016)
Observations	17,459	17,450	17,450	17,687	17,678	17,678
R-squared	0.011	0.095	0.095	0.004	0.094	0.095
Mean DV	27.39	27.39	27.39	0.662	0.662	0.662
Panel C: Work Outcomes (Males household heads / heads' spouses)						
	Hours/week			Worked last week		
TE (DD)	0.738	1.147	1.369*	-0.002	-0.002	0.001
	(0.759)	(0.783)	(0.753)	(0.008)	(0.007)	(0.007)
Observations	15,505	14,747	14,747	15,777	15,010	15,010
R-squared	0.006	0.092	0.092	0.002	0.074	0.075
Mean DV	47.86	47.86	47.86	0.949	0.949	0.949
Controls	NO	YES	YES	NO	YES	YES
Municipality FE	NO	YES	YES	NO	YES	YES
Group Trend	NO	NO	YES	NO	NO	YES
Clusters	290	290	290	290	290	290

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: The table presents OLS estimates for a difference-in-difference model. The coefficients represent differential changes in labor supply before and after the program between exposed and non-exposed children. Standard errors, clustered at the municipality level, are presented in parentheses. Panel A presents treatment effects concerning aggregate data at the household level. Panels B and C, present treatment effects regarding employment for females heads of household or spouses and males heads of household or spouses, respectively.

Table 2.5. Effects on Self-employment: Adult Females

	(1)	(2)	(3)	(4)	(5)	(6)
	Self-employed			**Works at home**		
TE (DD)	0.046**	0.042**	0.034*	0.022	0.020	0.004
	(0.018)	(0.017)	(0.019)	(0.016)	(0.014)	(0.015)
Observations	11,117	11,116	11,116	6,723	6,723	6,723
R-squared	0.004	0.121	0.121	0.015	0.130	0.132
Controls	NO	YES	YES	NO	YES	YES
Municipality FE	NO	YES	YES	NO	YES	YES
Group Trend	NO	NO	YES	NO	NO	YES
Clusters	279	279	279	254	254	254
Mean DV	0.463	0.463	0.463	0.123	0.123	0.123

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: The table presents OLS estimates for a difference-in-difference model. The coefficients represent differential changes in self-employment rate before and after the program between female head of households from exposed and non-exposed children. Standard errors, clustered at the municipality level, are presented in parentheses for adult females. The dependent variable is denoted as 1 if the head of household is self-employed and 0 if they did not report working the week preceding the survey.

Table 2.6. Adult Females: Heterogeneous Treatment Effects by Counterfactual Attendance Rate

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES		**Hours/week**			**Worked last week**	
TE (DD)	-5.684	3.156***	4.117***	-0.089	0.043***	0.043**
	(5.083)	(0.744)	(0.930)	(0.101)	(0.016)	(0.019)
TE x Attendance rate (DDD)	9.471			0.150		
	(5.755)			(0.114)		
TE x 1[Attendance rate<0.8] (DDD)		-4.120*			-0.028	
		(2.339)			(0.045)	
TE x 1[Attendance rate<median] (DDD)			-4.153**			-0.012
			(1.697)			(0.034)
Observations	14,563	17,450	17,450	14,750	17,678	17,678
R-squared	0.113	0.096	0.098	0.111	0.096	0.097
Clusters	288	289	289	289	290	290
Mean DV	27.39	27.39	27.39	0.662	0.662	0.662
Mean Covariate	0.853	0.154	0.423	0.853	0.154	0.423
1st Decile Covariate	0.659			0.659		
9th Decile Covariate	0.964			0.964		
TE at Percentile 10	0.561			0.0102		
p-val	0.714			0.737		
TE at Percentile 90	3.443***			0.056***		
p-val	0.002			0.007		
TE at CV=1		-0.964	-0.0357		0.0155	0.0305
p-val		0.664	0.978		0.706	0.247

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: The table presents OLS estimates for a triple-difference model. Standard errors, clustered at the municipality level, are presented in parentheses. The coefficients in the first row represent treatment effects when the relevant covariate equals 0 (DD). Interactions, located in the second, third and fourth rows denote differential treatment effects with respect to the TE presented in row 1 (DDD). Columns (1) and (4) report heterogeneity by counterfactual predicted attendance rate based on a probit model estimated for the 2004 sample. Columns (2) and (5) report heterogeneity for adult females belonging to households from inframarginal ($1[Attendance < 0.8] = 0$) and marginal ($1[Attendance < 0.8] = 1$) children. Columns (3) and (7) report heterogeneity for adult females belonging to households with children whose attendance rate is above the median ($1[Attendance < median] = 0$) and below the median.

Table 2.7. Adult Females: Heterogeneous Treatment Effects by Access to Credit

	(1)	(2)	(3)	(4)	(5)	(6)
		Hours/week			**Worked last week**	
TE (DD)	4.074***	3.485***	3.839	0.085***	0.079***	0.083*
	(1.335)	(1.249)	(2.368)	(0.025)	(0.027)	(0.045)
TE x # branches per 100000 people (DDD)	-0.073	-0.052	-0.078	-0.004*	-0.004*	-0.004**
	(0.109)	(0.111)	(0.116)	(0.002)	(0.002)	(0.002)
Observations	12,818	12,809	12,809	13,011	13,002	13,002
R-squared	0.007	0.045	0.051	0.003	0.049	0.057
Controls	NO	YES	YES	NO	YES	YES
Municipality FE	NO	YES	YES	NO	YES	YES
Group Trend	NO	YES	NO	NO	YES	NO
Area-cohort-year FE	NO	NO	YES	NO	NO	YES
Clusters	98	98	98	98	98	98
Mean DV	0.662	0.662	0.662	0.662	0.662	0.662
Mean # branches per 100000 people	9.341	9.341	9.341	9.341	9.341	9.341

*** $p < 0.01$, ** $p < 0.05$, $*p < 0.1$

Note: The table presents difference-in-difference estimates (DD) and triple-difference estimates (DDD) in the first and second row, respectively. The number of financial branches per 100,000 individuals in each municipality is used as a third source of variation. Data regarding financial branches corresponds to 2005, the year before the program's implementation, and is only available for the municipalities that are province capitals. The sample for these regressions accounts for one-third of the clusters' sample but two-thirds of the total observations. Standard errors, presented in parentheses, are clustered at the municipality level.

2.12 Acknowledgements

Chapter 2, is currently being prepared for submission for publication of the material. Vera Cossio, Diego ”Dependence or constraints? Cash transfers and female labor supply”.

Chapter 3

Access to Credit and Productivity

Abstract

Recent research estimating the causal impacts of microfinance finds modest population-level effects but highlights the presence of heterogeneous effects. We develop an empirical framework to analyze heterogeneity in the effects of micro-credit, which we implement in the context of one of the largest micro-credit programs in developing countries. We argue that in the case of credit-constrained households, cross-household variation in the marginal returns to capital –i.e., the shadow price from relaxing the budget constraint– is mainly driven by variation in total factor productivity (TFP). We modify standard control-function methods for estimating production functions by using household beliefs about future profits to proxy for unobserved productivity, instead of traditional proxy variables which may not be valid in the context of constrained households. Using five years of pre-program data, we apply our framework to recover TFP for all potential borrowers, and exploit quasi-experimental variation in the timing and size of the program to show that high-productivity households benefited the most from the credit-supply expansion: annual profits substantially increased (between TBH 0.7-2 with respect to one additional TBH of total credit), mostly driven by non-agricultural businesses. In contrast, we find no detectable impacts on profits for low-productivity households. Heterogeneity is even stronger among households with pre-existing non-agricultural businesses: We document increases in profits of TBH 1.8-4.1 per one additional TBH of total credit for high-productivity

households, which appear to be driven by increases in business assets. The findings suggest that improved screening and targeting could greatly magnify the impacts of credit expansions.

3.1 Introduction

Research from a range of contexts documents that small businesses in developing countries have high returns to capital grants (de Mel et al. (2008), McKenzie and Woodruff (2008), Kremer et al. (2010), Fafchamps et al. (2014), Blattman et al. (2014), McKenzie (2017), and Hussam et al. (2017)), and that programs involving transfers of productive assets to ultra-poor households are promising tools for poverty alleviation (Bandiera et al., 2017; Banerjee et al., 2015). Given such high returns, it is natural to conclude that financial frictions might pose significant barriers to entrepreneurship and firm growth. However, it has been harder to find broad empirical support for credit interventions such as microfinance catalyzing transformative business growth (Banerjee et al. (2015) and Meager (2016)). While the typical borrower's business appears not to grow more profitable in response to credit, this may hide substantial heterogeneity in the business returns to credit. Research has shown, for example, that entrepreneurs with pre-existing businesses do indeed experience sustained growth (Banerjee et al., 2015), and that the effects of credit are concentrated on the right tail of the distribution of profits (Banerjee et al., 2015; Crpon et al., 2015). Whether these businesses belong to highly productive households, has not yet been examined.

In this paper we develop an empirical framework to investigate heterogeneous returns to credit. Specifically, we ask whether cross-sectional heterogeneity in productivity predicts the returns to credit. We begin by developing a simple model of constrained households to show that the shadow price of capital is an increasing function of household productivity (TFP). We then apply our framework to the context of Thailand to assess the extent to which high-productivity households are more likely to increase profits and use program resources to boost business investment.

Our analysis requires three crucial components. First, we require quasi-exogenous variation in access to credit. For this, we use exposure to the Million Baht Program, one of the largest credit-expansion program of its kind, which began in Thailand in 2001.[1] We follow Kaboski and Townsend (2012), who exploit the fact that each program village received the same amount of funds from the central government to lend to local households, independent of village size. Thus we can compare villages before versus after the implementation of the program, by inverse village size. Second, we require detailed household panel data with enough pre-intervention observations to estimate TFP. Here, we also follow (Kaboski and Townsend, 2012) and use an unusually long panel, the Townsend Thai Project (Townsend, 2007b,a), which follows 960 households from 64 villages. Importantly, the data includes information on assets, inputs, revenues and profits for all household businesses, with five pre-intervention observations per household from the years 1997-2001. Finally, we require a credible way to measure household TFP. One problem when estimating production function that is well-known in the literature is that a household's investment decisions may be affected by time-varying shocks to productivity that are unobservable to the econometrician (Olley and Pakes, 1996). We propose a novel method in which we use data on household beliefs about future business conditions to proxy for the household's productivity shock. Our beliefs-based method is particularly attractive for settings like ours where households may be credit constrained (Shenoy, 2017).

Armed with these three tools, we first use the pre-program data to recover estimates of household productivity for all potential borrowers. We then combine the cross-household variation in productivity with the cross-village variation in the size and rollout of the program to test for productivity-based heterogeneity in the effects of the credit expansion.

Consistent with Kaboski and Townsend (2012), we find that indeed, villages with large inverse village sizes experience a large increase in short term credit following the implementation

[1] Concretely, the Thai government disbursed approximately USD 1.8 billion to 77,000 villages which represented over 95% of the total villages in Thailand. Because of its scale and policy relevance, a large body of research has analyzed different dimensions of the program. Kaboski and Townsend (2012) and Kaboski and Townsend (2011) provide reduced form and structural assessments of the impact of the program, respectively.

of the program, relatively to the baseline periods. Interestingly, the effects on program credit are not different for high versus low productivity households. This suggests that the village fund committees who decided how to allocate the credit did not direct more credit to higher productivity households, which is consistent with evidence of misallocation based on connections with local leaders (Vera-Cossio, 2018). One implication of this result is that potential heterogeneity in downstream outcomes is unlikely to be explained by differential access to program credit.

Turning to the reduced-form estimates of the effects of the program, we find strong patterns of heterogeneity by baseline productivity. First, we find no detectable impacts of the program on household income or business profits for low productivity households. However, the picture is quite different for high productivity households – they experience increases in total household income, coming largely from household enterprise profits. Moreover, the increase in profitability is driven almost entirely by non-agricultural businesses rather than farm profits. One interpretation of this result is that in the Thai context, credit constraints aren't as binding for agricultural businesses, perhaps due to differences in collateralizability of farm versus non-farm assets or due to pre-existing targeted agricultural lending programs.[2]

Next, we show that household TFP predicts larger treatment effects, restricting to the subsample of households with a pre-period non-agricultural business. We find evidence that for high-productivity households, village fund credit crowds in other types of borrowing. While high and low productivity households obtain similar amounts of village fund credit, total short-term borrowing increased more for high-productivity households relative to low-productivity households. Consequently, we find that among pre-program business owners, high-productivity households are better able to use the village credit to increase profits. This increase in profitability appears to be driven by an immediate increase in assets, rather than increased inventories and wage expenses.

In order to interpret the magnitude of these effects, we use the quasi-experimental

[2] Agriculture-oriented lenders are prominent in the context of rural Thailand. For instance, before the program's implementation, the Bank for Agriculture and Agricultural Cooperatives(BAAC) provided agricultural loans in all the sample villages.

variation in program exposure to instrument for total short-term credit. We find that high-productivity business owners exhibit annual returns to credit of the order of TBH 1.8-4 per one additional TBH of credit. Our estimates are consistent with evidence of high returns to credit in Morocco (Crpon et al., 2015), large annual returns to cash/asset grants in Mexico (McKenzie and Woodruff, 2008) and high returns to cash grants for entrepreneurs with high business growth potential in India (Hussam et al., 2017). These returns coincide with increases in assets of the order of TBH 4-7 per one additional TBH of credit. Back of the envelop calculations suggests that the implied annual rate of return of such investments ranges between 48-58%, way above the average annual interest rate associated to program loans (7%). These rate of returns are consistent with other estimates provided in the literature such as 39.6% annual for pre-existing firms winning a business plan competition in Nigeria (McKenzie, 2017), or estimates as large as 66-70% annual for the case of SMEs in Sri-Lanka (de Mel et al., 2008).[3]

We show that these results are robust in two ways. First, we use an alternate fixed effects-based approach to estimate pre-period household TFP and show that the results are qualitatively quite similar. Second, we present several approaches to impute total labor inputs to use in our estimation of household TFP.[4] We show that our main findings are robust to the inclusion of number of workers as a measure of labor in the production function estimation, and to estimating a production function in per-capita terms.

Our results contribute to the literature measuring the effects of credit supply expansions in developing countries ((Karlan and Zinman, 2010)- The Philippines, India (Banerjee et al., 2015), Morocco (Crpon et al., 2015), Mongolia (Attanasio et al., 2015), Bosnia (Augsburg et al., 2015), Mexico (Angelucci et al., 2015), Ethiopia (Tarozzi et al., 2015)). We document a new empirical result – that the most-productive households benefit the most from a credit expansion program. These findings complement previous studies which have documented evidence of heterogeneity

[3]We obtain these values by multiplying the monthly returns reported in the papers by 12.

[4]Unfortunately, the Townsend Thai Project annual data does not track total labor inputs in household businesses (i.e., time use by business activity). It only contains measures of the number of workers hired for non-agricultural businesses and the number of households members whose main occupation is to work in household businesses.

based on observable characteristics such as pre-period business ownership. We also show that household TFP is predictive of larger treatment effects even within these subpopulations. However, we find no correlation between credit supply and household TFP in our setting. One implication is that improved screening and targeting could greatly magnify the impacts of credit expansions. This could potentially entail improvements in externally identifying entrepreneurs (see Fafchamps and Woodruff (2017), Hussam et al. (2017), and Mckenzie and Sansone (2017)). Alternatively, financial institutions could try to design better screening mechanisms for self-targeting (Beaman et al., 2014).

Finally, our paper is related to the large body of literature aiming to estimate production function parameters and TFP (Olley and Pakes, 1996; Levinsohn and Petrin, 2003; Ackerberg et al., 2015; Shenoy, 2017).Shenoy (2017) argues that the assumptions typically made in the literature are likely unsuitable under credit constraints. We propose a novel implementation of the control-function approach using beliefs about future profits as a proxy variable, rather than intermediate inputs which require assuming frictionless adjustment in order to appropriately proxy for productivity. By using beliefs, we do not have to assume that households are not constrained and we do not have to impose a functional form for the process through which productivity evolves, as is usually the case in dynamic-panel approaches (Blundell and Bond, 2000).

The rest of the paper proceeds as follows. Section 3.2 provides and overview of the empirical context and the data. Section 3.3 presents a simple framework of credit supply expansions under credit constraints and also outlines our production function estimation methodology. Section 3.5 presents the core first stage and reduced form results, while Section 3.6 presents IV estimates. Finally, Section 3.7 concludes.

3.2 Context and Data

We study the heterogeneous household impacts of the Million Baht Program in the 64 villages of the Townsend Thai Project (Townsend, 2007b,a). Under the Million Baht program, the Thai government disbursed approximately USD 1.8 billion to 77,000 villages starting in 2001.[5] Our empirical strategy is based on the work of Kaboski and Townsend (2012), hereinafter KT, and exploits the unique implementation of the program to facilitate identification of its causal effects. Notably, the government disbursed exactly TBH 1,000,000 to each village regardless of size, wealth or location.[6] As such, inhabitants of small villages stood to receive more credit, on average, than residents of larger villages. In general, most of the credit was lent on a short-term (less than or equal to 12 months) basis, and because any funds repaid to the village fund committees were meant to be used to finance follow-on lending activities, the program could be viewed as a permanent supply shock to local short-term credit.[7]

Kaboski and Townsend (2012) provide quasi-experimental evidence of the effects of the program on household consumption and productive activities. Concretely, they document that short-term borrowing increased on the order of 1-for-1 with respect to the size of credit injection induced by the program, leading to even larger effects on consumption (increases of 1.7 TBH per TBH injected by the program). While most models of credit frictions would predict an increase in business investment and profits, the average effects of the program on productive activities are rather small. Other studies finding a similar pattern suggest that there is important heterogeneity based on observable characteristics (Banerjee et al., 2015). Yet, an empirical assessment of the importance of unobserved heterogeneity –i.e., managerial ability, or, more generally, productivity– has not been provided. In this paper we build on previous work by analyzing heterogeneity in the effects of the program on productive activities based on

[5] See Kaboski and Townsend (2012) for a detailed description of the program.

[6] Subject to each village successfully forming a village fund committee, the body which would ultimately manage the funds and make credit decisions along with loan collections.

[7] However, by 2004 several village fund committees had gone bankrupt due to mismanagement or default outbreaks, spurred by powerful members of the village.

pre-program household productivity.

We focus on the Thai context for three reasons: First, the Thai context provides arguably exogenous variation in the timing and size of the program to identify the effects of the program on household outcomes. Second, cross-village variation in the size of the program allows us to capture enough heterogeneity in household productive characteristics among program borrowers; in small villages which receive large per-capita program funds, both high and low productivity households may borrow. Third, the implementation of the program overlaps with the availability of a long panel dataset, the Townsend Thai Project, which records extremely detailed household records for 960 households from 64 villages in 4 Thai provinces. The nature of the data is unique in its comprehensiveness and panel length, which allows us to exploit the detailed, repeated nature of the household observations to implement modern panel-data methods to characterize households in terms of pre-program productivity and other productive characteristics.

Table 3.1 presents summary statistics for the study sample. Two important characteristics are worth emphasizing. First, household economic performance involves a variety of economic activities. While on average, higher shares of household operating income correspond to farming and wage work outside the household, 35% of households have an operating off-farm business. Second, even before the program, access to credit was common. Over two-thirds of households borrowed either from institutional or informal lenders. Moreover, 50% of households report having an outstanding loan with institutional lenders such as the state-owned Bank of Agriculture and Agricultural Cooperatives (BAAC), commercial banks and other local cooperatives or village organizations.

3.3 A simple theoretical framework

In this section we propose a simple theoretical framework to characterize the households who are able to best convert increased credit supply into business profits. We argue that in the presence of credit constraints, cross-household variation in the marginal return to capital –i.e., the shadow price from relaxing the budget constraint– is mainly driven by variation in total factor productivity (TFP). In order to illustrate this point, we start by analyzing a simple static profit-maximizing problem of a household or business facing a credit constraint.

Households are different in terms of total factor productivity ($TFP = A_i$), and combine fixed capital K, intermediate inputs or working capital M and labor L to produce output Y. Consider a Cobb-Douglas production function ($Y = A_i K^{\alpha_K} M^{\alpha_M} L^{\alpha_L}$),[8] then each household maximizes profits subject to a budget constraint:

$$\max_{K,M,L} A_i K_i^{\alpha_K} M_i^{\alpha_M} L_i^{\alpha_L} - p_K K_i - p_M M_i - p_L L_i \tag{3.1}$$

subject to

$$p_K K_i + p_M M_i + p_L L_i \leq B_i \tag{3.2}$$

Where B_i denotes the total budget available to household i, and includes both wealth and credit. We allow heterogeneity in this dimension to capture differences in wealth as well as access to credit across households. Input prices (p_K, p_M, p_L) are normalized with respect to the price of output. Let λ_i denote the LaGrange multiplier associated to the budget constraint (3.2). Thus, λ_i represents the shadow value of a marginal increase in household i's budget (B_i): the marginal return to capital. Thus, if credit expansion programs effectively modify the availability of resources B_i, then heterogeneity in λ_i captures heterogeneity in the ability of a household to benefit from increases in the supply of credit.

Combining the first order conditions corresponding to the choice of each input, it is possible to show that an optimal solution implies:

$$A_i B_i^{\alpha_K + \alpha_M + \alpha_L - 1} \kappa = 1 + \lambda_i \tag{3.3}$$

[8]The theoretical predictions highlighted in this sections do not depend on the number of inputs and hold for concave production functions.

As κ is strictly positive, λ_i is an increasing function of total factor productivity (A_i).[9] Moreover, with decreasing returns to scale ($\alpha_K + \alpha_M + \alpha_L < 1$), λ_i is decreasing in B_i. In words, households benefit more from relaxing the budget constraint if productivity is high and if wealth or credit availability are low. In the context of a technology with constant returns to scale, the budget constraint is irrelevant, and only heterogeneity in TFP drives heterogeneity in the shadow value of capital.

3.4 Empirical strategy

Kaboski and Townsend (2012) exploit variation in the timing and size of the program to estimate its causal effects on productive outcomes. In particular, they compare changes in outcomes before and after 2001 corresponding to villages with high per-capita expected credit supply (or high inverse village size, $invHH$) to those with low per-capita expected credit supply (or low inverse village size). This approach would lead to causal identification of the effects of the program under the assumption that there were not time-varying shocks that differentially affected small and large villages, and could potentially be related to outcomes. The authors argue that the spatial distribution of village size is as if random and validate the identification assumptions with several robustness checks. We build on their empirical approach by analyzing the heterogeneous effects of the program motivated by our theoretical framework.

Our aim is to understand if households with higher λ_i do in fact benefit more from the increase in the supply of credit induced by the Million Baht Village Fund program. Let $\lambda_{i,n}$ be the household's shadow value of capital for household i in village n, corresponding to the baseline periods. While we do not observe $\lambda_{i,n}$, our theoretical framework suggests that baseline productivity $A_{i,n,t}$ captures important variation in the returns to capital. Thus, in our empirical

[9] $\kappa = \left(\frac{1}{\alpha_K+\alpha_M+\alpha_L}\right)^{\alpha_K+\alpha_M+\alpha_L-1} \left(\frac{\alpha_K}{p_K}\right)^{\alpha_K} \left(\frac{\alpha_M}{p_M}\right)^{\alpha_M} \left(\frac{\alpha_L}{p_L}\right)^{\alpha_L}$

analysis we aim to estimate the heterogeneous reduced-form effects of the program following:

$$
\begin{aligned}
y_{i,n,t} =& \delta_1 invHH_n \times Post_t + \delta_2 invHH_n \times Post_t \times \text{High A}_{i,n} + X_{i,n,t}\Gamma \\
&+ \delta_3 \text{High A}_{i,n} + \theta_t \times \text{High A}_{i,n} + \theta_t + \theta_n + e_{n,t}
\end{aligned} \tag{3.4}
$$

Here, n indexes the village, t indexes the year, and i indexes households. High $\text{A}_{i,n}$ is an indicator that identifies households in the top-third of the TFP distribution, within each village. We mainly focuss on rankings rather than levels to attenuate potential measurement error as we estimate $A_{i,n}$ (see Section 3.4.1) $Post_t$ is an indicator that identifies post-program years. We allow for TFP-specific time trends and include a $1 \times I$ vector of covariates $X_{i,n,t}$ (including household composition, age, and education), village (θ_n) and year fixed effects (θ_t). The coefficients of interest are δ_1, $\delta_1 + \delta_2$, and δ_2, they represent the reduced-form effects of the program for low productivity households, high-productivity households, and the differential effect for high-productivity households, respectively.

Note that while identification of the average treatment effect relies on parallel trends between large and small villages, the identification of heterogeneous effects based on productivity requires that the parallel trends assumption holds for both high and low productivity households. In order to test the validity of such assumption and provide dynamic assessment of the program effects, we report estimates corresponding to a flexible-difference in difference specification which is separately estimated for high and low productivity households:

$$
y_{i,n,t} = \sum_{\tau=1997, \tau \neq 2001}^{\tau=2006} \delta_\tau invHH_n \times \mathbf{I}[t = \tau] + X_{i,n,t}\Gamma + \theta_t + \theta_n + e_{n,t} \tag{3.5}
$$

While the reduced-form estimates are important to test the presence of productivity-based heterogeneity, we also provide IV estimates of the local average treatment effect (LATE) of an additional TBH of credit on profits corresponding to households who were induced to borrow more due to the program. We report estimates of the following structural specification which is based on the work of Kaboski and Townsend (2012):

$$\begin{aligned} y_{i,n,t} =& \beta_1 STCR_{i,n,t} + \beta_2 \text{High A}_{i,n} \times STCR_{i,n,t} + X_{i,n,t}\Gamma \\ & + \beta_3 \text{High A}_{i,n} + \theta_t \times \text{High A}_{i,n} + \phi_n + \phi_t + \varepsilon_{i,n,t} \end{aligned} \tag{3.6}$$

with first stage:

$$STCR_{i,n,t} = \sum_{\tau=2002}^{\tau=2006} \delta_\tau invHH_n \times \mathbf{I}[t=\tau] + X_{i,n,t}\Sigma + \theta_t + \theta_n + e_{n,t} \tag{3.7}$$

Here, the parameters of interest are β_1, which captures the LATE of short-term credit on business profits for low-productivity households, β_2 which captures the differential effect of credit between high and low productivity households, and $\beta_1 + \beta_2$ which captures the LATE for high-productivity households. Note that we also need to instrument for High $\text{A}_{i,n} \times STCR_{i,n,t}$. Because High $\text{A}_{i,n}$ is predetermined, we simply construct the first stage by pre-multiplying all terms in the standard first stage by High $\text{A}_{i,n}$. In the structural equation, this yields two endogenous regressors, ($STCR_{i,n,t}$ and High $\text{A}_{i,n} \times STCR_{i,n,t}$) and two sets of instruments.

Two important issues should be noted. First, we use the variation induced by the timing and relative size of the program to instrument for total short-term credit as opposed to program credit only. We do so to allow for potential responses in local credit markets that are likely to occur in this setting. [10] Second, our estimates provide an approximation to the financial returns to short-term credit only under the assumptions of no general equilibrium effects. We argue that while this assumption is strong,[11] the presence of general equilibrium effects would imply that

[10] For instance, (Vera-Cossio, 2018) shows that there is redistribution of program resources through informal credit markets.(Kinnan and Townsend, 2012) show that households rely on indirect access to formal credit to smooth consumption and investment decisions.

[11]For instance, households may consider the program as a permanent increase in the availability of credit in

households who did not borrow would still benefit from the program and hence our estimates would provide lower-bounds of the returns to credit.

3.4.1 Production function estimation

Central to our analysis is the measurement of productivity $A_{i,n}$ for each potential borrower, which typically requires the estimation of a production function. We model log value added ($va_{i,t}$), aggregated across all household productive activities,[12] as a function the stock of fixed capital $k_{i,t}$,[13] productivity shocks which are observed by the household but not by the researcher $\omega_{it} = log(A_{it})$, and un-expected production shocks $\varepsilon_{i,t}$ which are neither known by the household nor by the researcher.[14]

$$va_{i,t} = \beta_0 + \beta_k k_{i,t} + \omega_{it} + \varepsilon_{i,t} \tag{3.8}$$

We are interested on estimating ω_{it} for each household, which represents variation in value-added conditional on capital.[15] That is, we aim to capture differences across households in their ability to generate value added. We allow productivity to evolve following two sources of variation: foreseen variation based on previous realizations (e.g., $\omega_{i,t-1}$) and unforeseen shocks to productivity $\zeta_{i,t}$. The empirical challenge is to consistently estimate β_k, which is essential to back out ω_{it}. In order to do so, we need to tackle two potential problems. First, households may

the local economy (Kaboski and Townsend, 2011) or there could be general equilibrium effects affecting wages Kaboski and Townsend (2012).

[12]They include cultivation, livestock, production of livestock produce and off-farm family business. Value added is measured as total revenues net of costs from using inputs, other than capital and labor. For instance, we subtract the value of fertilizer, seeds, feed, merchandise and fuel (among others) from total gross household revenues.

[13]The stock of capital is measured as the stock of fixed assets corresponding to farm and non-farm businesses.

[14]We do not have detailed data regarding labor hours, so we focus on the relation between value-added and capital. However, we present robustness checks based on estimations that use the number of workers hired for off-farm businesses and the number of adults in the households as a proxy for potential labor.

[15]We use a value-added function over a gross revenue function as households may have different sources of income and use output from one occupation to obtain outcome for another. For instance, a farmer may produce some crops for sale but may use part of the harvest for feed for its livestock. Without a systematic accounting process, a gross revenue approach could lead to double accounting.

respond to unexpected production shocks $\varepsilon_{i,t}$ and productivity shocks $\zeta_{i,t}$ by adjusting capital. Second, households may optimally decide their investment decisions in order to accommodate foreseen variation in productivity ω_{it} (Olley and Pakes, 1996). Both sources of endogeneity may lead to biased OLS estimates of β_k. Ideally, we would rely on household-level experimental variation in the stock of fixed capital to compute β_k. While such a source of variation is not available in our context, the richness and length of our panel dataset allow us to go a long way in reducing these concerns.

In order to tackle the first problem, we use the first lag of capital as a proxy for the stock of capital available to the household before experiencing unforeseen production and productivity shocks. By doing so, we prevent capital from responding to current shocks such that $\mathbf{E}[k_{i,t}, \varepsilon_{i,t}] = 0$ and ($\mathbf{E}[k_{i,t}, \zeta_{i,t}]$). This approach is consistent with models in which there is time to build related to productive capital (Kydland and Prescott, 1982), and is consistent with evidence of lumpy investments in Thai villages (Samphantharak and Townsend, 2010). Tackling the second problem requires controlling for unobserved variation in ω which is correlated with capital choices. We propose two approaches that rely on different identification assumptions to overcome this issue.

Fixed-effects approach

A first approach would be to impose some structure in the process through which ω_{it} evolves over time. In order to do so, we assume that variation in productivity is explained by a time-invariant component which is correlated with capital decisions, year-specific aggregate shocks, and a time-variant unforeseen shock which is experienced after households chose capital –i.e., $\omega_{it} = \bar{\omega}_i + \omega_t + \zeta_{it}$, with $\mathbf{E}[\bar{\omega}_i, k_{it}] \neq 0$ and $\mathbf{E}[\zeta_{it}, k_{it}] = 0$. This specification allows us to estimate (3.8) through a fixed-effects approach using the 5 years preceding the program, and use within-village rankings of the estimated $\hat{\bar{\omega}}_i$ to estimate equations (3.4) and (3.6).

While simple, this approach has two limitations. First, by not allowing the foreseen part of productivity to evolve over time, the fixed-effects approach rules out models in which households may accumulate knowledge or develop abilities which may allow them to more efficiently use capital in future periods. If the latter models are the main drivers of households behavior, then the fixed-effects approach may fail to fully account for the relation between capital and productivity. Second, even if a fixed-effects model is a good description of the true data-generating process, the identification of β_k will rely on within household variation in capital, which may be troublesome in contexts in which investment is lumpy and there is measurement error in capital. In such cases, fixed-effects estimates of productivity may end up absorbing most of the variation in the stock of capital.

Control function approach

A less restrictive approach for estimating β_k relies on the use of proxy-variables in order to control for variation in productivity (Olley and Pakes, 1996; Levinsohn and Petrin, 2003; Ackerberg et al., 2015). By doing so, these approaches allow productivity ω_{it} to evolve following less restrictive processes. Typically, the control function approaches use variation in the demand for intermediate inputs to proxy for variation in productivity, and assume that firms accommodate productivity shocks by freely adjusting these inputs.[16] While appealing, the use of this approach seems limited in the context of constrained households in developing countries. Shenoy (2017) shows that, in the context of liquidity constraints, households/firms may not be able to freely adjust intermediate inputs in order to accommodate productivity shocks, and thus variation in intermediate inputs may not fully capture variation in productivity.[17] In this paper, we propose a simple modification to the control-function approach in which we use household beliefs about future business conditions to proxy for variation in foreseen productivity.

[16]Traditional proxy variables are materials or electricity. The control function approach observes, that demand for intermediate goods, m_{it}, can be expressed as a function of the current capital stock and productivity, $m_{it} = m_{it}(K_{it}, \omega_{it})$. Under some assumptions, mainly a strictly monotonic relation between m and ω, the demand function can be inverted yielding $\omega_{it} = \omega(K_{i,t}, m_{i,t})$ (Levinsohn and Petrin, 2003).

[17]Shenoy (2017) proposes the use of dynamic panel methods that would be based on weaker assumptions regarding optimal household/firm behavior. However, relaxing such assumptions as in Blundell and Bond (2000), comes at the cost of imposing functional forms to the productivity process (typically, assuming that productivity follows an AR(1) process). Moreover, the implementation of such models requires long time series in order to avoid problems with precision. See Vera-Cossio (2018) for an empirical application of dynamic panel methods in rural Thailand.

We postulate that household's beliefs about business conditions in period t ($b_{i,t}$) are a function of capital and productivity shocks: $b_{i,t} = b(K_{i,t}, \omega_{i,t})$. Thus, our ability to effectively use variation in $b_{i,t}$ to proxy for variation in $\omega_{i,t}$ relies on the idea that if we observed different beliefs across households with similar capital, it should be the case that households with higher beliefs are households with higher productivity. To the extent that households incorporate variation in productivity into their beliefs in a frictionless way, it would be possible to invert the relation between beliefs and productivity and write down ω as function of household beliefs and capital ($\omega_{i,t} = \omega(K_{i,t}, b_{i,t})$). Note that while this approach is inconsistent with models of cognitive rigidities in the formation in beliefs (Handel and Schwartzstein, 2018), our approach does not assume frictionless adjustment of inputs and provides a novel alternative for the use of choice-based models in the contexts of credit constrained households.

Under these assumptions, our estimation procedure is similar to the two-stage approach proposed by Levinsohn and Petrin (2003). First, we use third-order polynomials of $k_{i,t}$ and $b_{i,t}$ to recover variation in value added that is explained by capital, and household beliefs:

$$\hat{v}_{i,t} = \hat{\delta}_0 + \sum_{j=0}^{3}\sum_{l=0}^{3} \hat{\delta}_{jl} k_{i,t}^{j} b_{i,t}^{l} \tag{3.9}$$

Second, for a given value of β_k, we can recover estimates of productivity shocks:

$$\hat{\omega}_{i,t}(\beta_k) = \hat{v}_{i,t} - \beta_k k_{i,t}$$

Next, we allow non-parametric persistence in productivity by assuming ω follows a first-order Markov process ($\omega_{it} = \mathbf{E}[\omega_{i,t}|\omega_{i,t-1}] + \zeta_{i,t}$), and estimate $\mathbf{E}[\hat{\omega}_{i,t}|\hat{\omega}_{i,t-1}]$ by regressing $\hat{\omega}_{i,t}(\beta_k)$ on a third-order polynomial of the previous realization of the shock ($\hat{\omega}_{i,t-1}(\beta_k)$). Finally, β_k^* is is chosen to minimize the sum of squared residuals:

$$\min_{\beta_k^*} \sum_t \sum_i \left(v_{it} - \beta_k^* k_{it} - E\left[\hat{\omega}_{it}|\hat{\omega}_{it-1}\right]\right)^2 \tag{3.10}$$

At the end of the procedure, we average the estimates $\hat{\omega}_{i,t}$ over the pre-intervention periods and generate within-village rankings of household productivity.[18]. We then use these rankings to analyze heterogeneity in the effects of the program.

We implement our empirical strategy using household projections of income at time t, which were measured in $t-1$ and are highly predictive of value-added (See Appendix Table C.1).[19] Note that, in our setting, while lagged beliefs only capture variation in foreseen productivity shocks, there is no need to impose additional assumptions regarding the timing of capital other than being predetermined with respect to $\zeta_{i,t}$.[20] Table 3.2 reports estimates of β_k under different methods and provides summary statistics of $\hat{\omega}$ averaged across the 5 pre-program periods, which are our main proxies for household productivity A. While the fixed-effects approach achieves low estimates of β_k and larger estimates of productivity than the control-function approach, the implied within-village productivity rankings are similar across both methods (see Appendix Table C.2). To test the extent to which our productivity estimates capture meaningful information, Appendix Table C.3 shows that our estimates of productivity correlate with household characteristics that are usually associated with higher productivity, such as education. This pattern holds for both estimates of productivity: the fixed-effects and proxy-variable approach. Through the rest of the paper, we present evidence based on estimates from both approaches and rely on results that are robust across empirical strategies with different identification assumptions.

[18]Note that because we assume that capital is predetermined with respect to production shocks ($\varepsilon_{i,t}$) and to unforeseen innovations in productivity $\zeta_{i,t}$, identification is achieved under the following moment condition $\mathbf{E}[\zeta_{i,t}+\varepsilon_{i,t}|k_{i,t},k_{i,t-1},b_{i,t-1}]=0$ which is approximated by its sample analog corresponding to equation (3.10)

[19]The survey obtains information regarding income projections in *a)* a regular scenario, *b)* an adverse scenario and *c)* a good scenario. To account for differences in scale and volatility, we construct a measure of beliefs by dividing the difference in projected income between a regular and a bad scenario by the difference in projected income between the good and bad scenario.

[20]However, it is worth noting that in a more complicated model with flexible inputs such as labor, our approach would require using measures of beliefs that are based on information available to households after $\omega_{i,t}$ is realized, but before the final allocation of value-added and other flexible inputs are observed.

3.5 Reduced-form results

3.5.1 Effects on program and total short-term credit

We begin by analyzing the extent to which baseline productivity is captures heterogeneity in program borrowing and total short-term borrowing. For the sake of consistency with previous studies (KT), we focus our analysis on observations from 1997 to 2006, covering 5 years of pre and post program data. Figure 3.1 presents flexible difference-in-differences estimates of the effect of the rollout of the program on program (Panel A) and total short-term credit, for high and low productivity households (based on the proxy-variable approach). Consistent with Kaboski and Townsend (2012), we find that indeed, villages with large inverse village sizes experience a large increase in short-term credit following the implementation of the program, relatively to the baseline years. These increases are associated to an average loan size of TBH 16,000 for compliers (USD 360 at 2001 exchange rate).

We also find that baseline productivity in not predictive of program borrowing. Panel A from Figure 3.1 shows that differences in program participation in small villages (more per-capita resources) with respect to large villages (less per-capita program resources) are orthogonal to productivity. This pattern suggest that there was some degree of misallocation in terms of providing resources to the most productive households, and is consistent with evidence documenting targeting frictions in the program (Vera-Cossio, 2018). Interestingly, this result highlights a key feature of our research design: because identification comes from the relative size of the shock, we are able to observe high-and-low productivity households among program compliers. This strong "first-stage" for both high and low productivity households suggests that potential heterogeneity in downstream outcomes is unlikely to be driven by selection into the program. Results are similar in the case of productivity rankings based on the fixed-effects method (see Panel B from Table 3.3).

To analyze the extent to which program resources crowded in or crowded out other sources of credit, Panel B shows the reduced form effects of the program for high and low productivity households. Rather than crowding out other sources of credit, the program appears to have promoted complementarities across sources of credit. The figure shows point estimates that are larger than those associated to program credit (Panel A). While this result holds for high and low productivity households, the point estimates are larger in the case of high-productivity as the rollout of the program might have encouraged high-productivity households to borrow more from other sources of credit. However, these differences are not significant on average (see Column 2 from Table 3.3).

3.5.2 Effects on household income

While there are not differential effects in program credit, we document strong heterogeneity in household income. The top-left panel of Figure 3.2 shows that while total income does not seem to change for lower-productivity households, income increases for high-productivity households due to the program. In order to analyze the main source of this increase, we look at the effects of the program on wage income, farm profits and non-agricultural business profits. We find no evidence of differential effects on farm profits or wage income (see bottom panel). However, the picture is quite different when we focus on non-agricultural businesses. We find that business profits increased for high productivity households, and we fail to find responses in the case of low-productivity households (see top-right panel of Figure 3.2). Note that although the differences in the effects between high and low productivity households seem to slowly decay over time, they are quite strong and precise during the first two years of the program. This pattern is not surprising since baseline productivity may lose some predicting power as time passes. These patterns are qualitatively similar if we use a fixed-effects approach to recover baseline household productivity (see Appendix Figure C.1).

Panel A from Table 3.3 presents reduced-form estimates corresponding to the specification in equation (3.4), which capture the effect of an extra per-capita TBH of credit in a given village on household outcomes.[21] In terms of magnitudes, on average high-productivity

[21] We divided the point estimates from equation (3.4) by 1,000,000 to provide a TBH-to-TBH interpretation.

households experienced income increases of 2.4 TBH per additional per-capita TBH available in the village ($p < 0.10$). These results coincide with an increase in off-farm profits of similar magnitude, which is significantly different to the reduced-form effects for low-productivity households ($p < 0.05$). Panel B in Table 3.3 shows that the same patterns are observed when productivity is measured using the fixed-effects approach.

The former set of results show that non-agricultural family business drive the effects of the program on household profits. This result is consistent with evidence from India documenting sustained increase in profits for pre-exisiting businesses (Banerjee et al., 2015). One interpretation is that in the rural Thai context, credit constraints are not as binding for agricultural businesses, perhaps due to differences in collateralizability of farm versus non-farm assets or due to a pre-existing credit options targeting agricultural businesses. Indeed, over one-half of the households in the sample had access to institutional credit at baseline (see Table 3.1), mainly through the Bank of Agriculture and Agricultural Cooperatives (BAAC) but also through other agriculture-oriented lenders such as production credit groups (PCGs), and cooperatives. The results suggest that in the Thai context, a targeted policy oriented at alleviating constraints for non-agricultural businesses would have complemented the existing government-led agricultural programs.

3.5.3 Effects for owners of pre-existing businesses

While our results are consistent with previous evidence showing that pre-business ownership is a relevant source of heterogeneity (Banerjee et al., 2015), we document a novel result: there is meaningful heterogeneity based on productivity even among pre-existing business owners.[22] Figure 3.3 documents that business profits increased substantially for high-productivity households with preexisting businesses and not so for low-productivity households who were also business owners. Column 3 from Table 3.4 complements the graphical evidence by showing that there is significant productivity-based heterogeneity in the program effects on profits ($p < 0.05$). Consistent with our theoretical framework, the results show that there is a sizeable degree of

[22] We define business owners as households who hold business assets in the period preceding the program.

heterogeneity among entrepreneurs. Our results are consistent with previous evidence of heterogeneity based on observable characteristics in credit-expansion programs (Banerjee et al., 2015; Crpon et al., 2015; Banerjee et al., 2015). However, we build on the existing literature by showing that unobservable characteristics that are captured by productivity are highly predictive of business success in the aftermath of a credit expansion program.

Two complementary results are related to the differential effects on profits among business owners. First, high-productivity business owners increased total short-term credit differentially.This result is mainly explained by the program crowding in other types of credit since there is no productivity-based heterogeneity in program borrowing for this sub-sample (see Columns 1 and 2 from Table 3.4). Second, heterogeneity in the effects of profits seems to be mainly related to differential increases in business assets between high and low productivity households ($p < 0.10$) and not to wage spending or inventories. These results are robust to estimating productivity following the fixed-effects approach (see Panel B from Table 3.4), and to the exclusion of potential outliers.[23]

In order to understand the dynamics of the reduced-form effects on business assets, Figure 3.4 plots flexible difference-in-difference estimates for high and low productivity owners of pre-existing businesses. While low-productivity households do not increase business assets due to the program, high-productivity households start scaling up their businesses as early as 2001. One interpretation is related to expectations about future increases in the supply of credit that triggered early investments following the announcement of the program, which was one of the flagship policies of the incoming Government elected on January of 2001. Alternatively, a rapid adjustment to the new economic conditions induced by the program may allow to capture very short term effects.[24] Overall, the results suggest that high-productivity business owners were able to increase their profits by scaling up their businesses and complementing the resources

[23]We top coded the dependent variables with respect to the 99th percentile. Appendix Table C.4 shows that while the point estimates decrease after truncating the dependent variables, they are more precisely estimated.

[24]Since the annual resurveys are conducted starting on May of each year over a period of 6 weeks, it is likely that they are able to capture immediate responses in investments for villages with early program rollout.

obtained from the program with loans from other type of lenders.

3.5.4 Robustness

One limitation of our empirical analysis is that, due to data limitations, our productivity estimates do not account for the role of labor, and only capture variation in output conditional on the stock of capital. As a result, high-productivity households ($HighA$) are the ones that would generate more value added given a certain amount of productive capital, but may not be the ones that would generate more value added holding constant *both* capital and labor. However, our estimates of productivity would still capture economically meaningful and policy-relevant variation in contexts in which the effects of micro-credit programs on household profits are not likely to be driven by adjustments in labor markets. Our results suggests that such an scenario is likely to fit the Thai context.[25]

We report two robustness analyses that try to account for labor using rather rough proxies.[26] First, we replicate the control-function approach including the number of household members who reported working in household production as their main occupation plus the number of hired workers as a proxy for labor.[27]. Second, we replicate our analysis estimating the production function in per-capita values in order to account for household size which could be correlated with labor. Appendix Figure C.2 replicates our main results through these approaches. Though noisier, the patterns are still similar to those corresponding to our main empirical approach.

[25]Kaboski and Townsend (2012) fail to find average effects on household spending in labor and provide suggestive evidence of impacts on the probability of investment in agricultural assets.

[26]Ideally, we would want to observe information regarding time use. In particular, we would need information of the amount of hours allocated to household production by household members and the number of work hours by hired labor.

[27]We estimate a slightly modified version of our main approach using the two-stage procedure described by Ackerberg et al. (2015)

3.6 IV estimates of the returns to credit

In order to provide an approximation of the baht-to-baht relation between total short-term credit and household productive outcomes for program borrowers, we use the rollout of the program interacted with the program's size in each village to estimate the specification from equation (3.6). We emphasize that results corresponding to IV estimates are only suggestive; while the reduced-form effects are internally valid and provide estimates of the effects of the program on household outcomes, the instrumental-variable estimates only deliver causal estimates under additional assumptions. However, we argue that to the extent that general equilibrium effects affected borrowers and non-borrowers similarly within a village, our IV estimates provide us with a tool to compare the approximated financial returns to an extra unit of credit with other estimates in the literature.

We find that our IV estimates imply sizeable returns to credit for high-productivity households. First, we focus on the full sample. Table 3.5 reports LATE estimates of an additional TBH of credit on household income and profits. Columns (1) and (2) from Panel A show that for each additional TBH of short-term credit induced by the credit expansion program, high productivity households were able to generate between 1.6-2.3 additional TBH of income on a given year relative to low-productivity households, using truncated data (top 1%) and raw data respectively. These estimates imply annual returns to credit of 68-136% for high-productivity households (See bottom rows form Panel A) and rather small and insignificant negative returns for low-productivity households.

Second, we find even higher returns to credit when we focus on high-productivity business owners. The bottom panel of Table 3.6 shows that the effects on business profits are on the order of TBH 1.8-4 per additional TBH of credit on a given year. These effects are similar to the effects found by Crpon et al. (2015) in Morocco (2.4) and Banerjee and Duflo (2014) in the case of a targeted program for medium and large firms in India. Relative to the literature estimating the returns to cash grants, the returns to credit for Thai high-productivity business owners are as high as those of entrepreneurs who were identified as being of "high-growth potential" by their fellow villagers in India (330% annual, Hussam et al. (2017)).[28] and for returns to capital grants in Mexico of over 700% annual for firms which report being credit constrained (70% monthly, McKenzie and Woodruff (2008)). Table 3.6 also shows that there are neither meaningful effects on inventories nor expenditures on wage work, but that there are substantial increases in business assets in the order of TBH 3.7 to 7.1 per additional TBH of credit, in the case of high-productivity households. One potential explanation to such magnitudes is that, consistent with the idea of the program crowding in other sources of credit, the results suggest that households may have also used cash holdings to complement credit in financing a lumpy investment.

3.6.1 Implied rates of return to investments on business assets

Despite capturing the average annual increase in profits due to an extra unit of credit, our LATE estimates are not necessarily informative about the rate of return of household projects which, according to our results, imply expanding pre-existing businesses by acquiring non-agricultural business assets. The rate of return is an intrinsically policy-relevant parameter as household may ultimately evaluate the return of their projects against the cost of financing them.

In order to provide an approximation we simply re-scale our estimates of the effect of credit on profits, by dividing them by our estimates of the effect of credit on business assets (See the bottom panel in Table 3.6). The implied rates of returns range between 48% to 58% per annum, depending on whether we use the estimates using truncated data or raw data, respectively. Such magnitudes are smaller than the LATE estimates, yet substantially higher than the interest rates charged by the existing lenders in the Thai context: 7% per annum in the case of program loans, 12% for loans from BAAC bank and 22% for loans from informal lenders. The implied returns are consistent with other estimates provided in the literature such as 39.6% annual for pre-existing firms winning a business plan competition in Nigeria (McKenzie, 2017), or estimates

[28]Hussam et al. (2017) document returns to capital grants as high as 28% monthly

as large as 66-70% annual for the case of SMEs in Sri-Lanka (de Mel et al., 2008).[29]

While we find evidence of high rates of return to household investments for the most productive households, we also found that program borrowing seems orthogonal to productivity, suggesting misallocation. One implication of these results is that improved screening and targeting could greatly magnify the impacts of credit expansions. This could potentially entail improvements in externally identifying high-return entrepreneurs (see Hussam et al. (2017) and Mckenzie and Sansone (2017)). Alternatively, financial institutions could try to design better screening mechanisms for self-targeting (Beaman et al., 2014).

3.7 Concluding remarks and policy implications

In this paper we show that cross-household variation in productivity is an important predictor of heterogeneity in the effects of credit-expansion programs. Our empirical strategy relies on the unusual availability of three important pieces. First, we needed arguably exogenous variation in the supply of credit to identify causal effects. The Thai context provided us with variation from one of the largest credit-expansion programs ever implemented in developing countries: the Million Baht Village Fund Program (Kaboski and Townsend, 2012, 2011). The program's scale also provided us with enough power to conduct heterogeneity analyses. Second, we needed a rich dataset that allowed us to fully characterize potential borrowers in terms of pre-program productivity. The Townsend-Thai project (Townsend, 2007a,b) provided us with five years of pre-program data, which we exploited to implement panel-data econometric methods to back out estimates of household productivity. At the same time, it provided us with five post-program years which we use to analyze dynamics, and assess the predictive power of our productivity estimates and theoretical insights.

The third piece involves a suitable method to estimate household productivity. Popular methods for estimating production functions generally use variation in investment or intermediate inputs to non-parametrically control for unobserved productivity shocks (Olley and Pakes, 1996;

[29] We obtain these values by multiplying the monthly returns reported in the papers by 12.

Levinsohn and Petrin, 2003; Ackerberg et al., 2015). However, these methods assume frictionless adjustment of the proxy variables, which is unlikely to hold in the case of credit-constrained households (Shenoy, 2017). We propose a novel implementation of the control-function approach using beliefs about future profits as a proxy variable, which makes no assumptions regarding how investment and inputs are adjusted and allows for the presence of credit constraints.

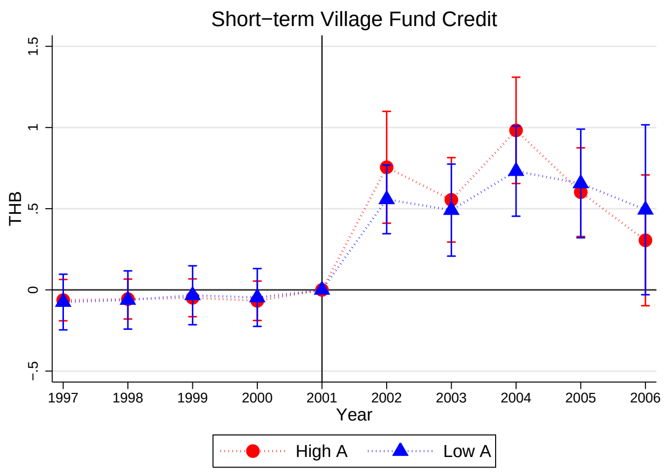
Our approach is not perfect, yet our main results are robust to alternative approaches with different assumptions. For instance, our approach does assume that beliefs are updated without systematic frictions We show that our results are similar when we measure productivity using a fixed-effects approach that does not rely on beliefs. Second, due to our inability to use time-use information to proxy for labor, our measures of productivity may capture also variation in labor. We propose two alternative approaches to incorporate rather imperfect measures of labor, and show that our main results are robust to these specifications.

Overall, our research design is quite unique in the literature and allows us to uncover a novel piece of evidence: that high-productivity households are best able to convert increased credit supply into profits and boost investment. This result is driven by heterogeneous increases in profits for non-agricultural businesses. Moreover, we also find that even among households with pre-existing non-agricultural businesses, there is a great deal of productivity-based heterogeneity in the returns to credit. In the case of high baseline productivity business owners, we document high returns to credit consistent with high-returns to capital grants in developing countries (de Mel et al., 2008; McKenzie and Woodruff, 2008; Hussam et al., 2017). We also find that these high returns coincide with households scaling up their non-agricultural businesses. Business assets increased almost immediately after program funds were first dispensed, and imply internal rate of returns in the range of 48 to 58% over a five-year period, which are consistent with estimates from cash-grant studies (de Mel et al., 2008; McKenzie, 2017).

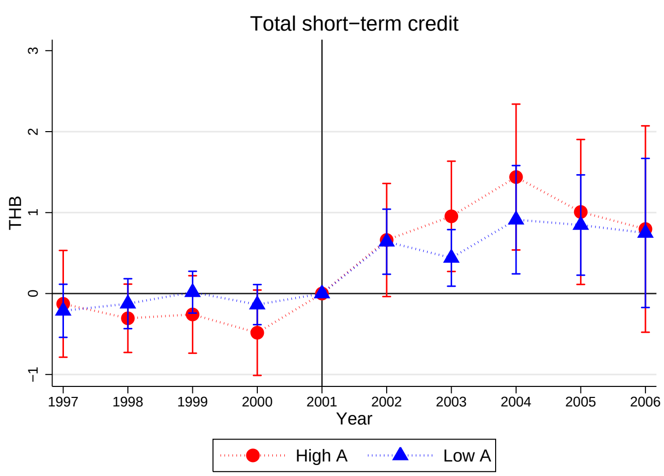
Our results highlight two important policy implications. First, our results suggests that large-scale credit interventions may provide resources to sectors facing stronger credit constraints. In our setting, we find evidence of high marginal returns to credit for high-productivity non-agricultural businesses and not so for farm businesses. Non-agricultural businesses were not the target of pre-existing agriculture-oriented lenders such as the state-owned Bank for Agriculture and Agricultural Cooperatives (BAAC). While our results speak directly to the context of rural and peri-urban Thailand, the lessons may still apply to settings in which policy efforts to alleviate financial frictions are mostly focussed in one particular sector.

Second, our results imply that improved screening and targeting could greatly magnify the impacts of credit expansions. While we document high-returns to credit for high productivity households, we also find that program borrowing was orthogonal to baseline productivity. Thus the policy challenge involves effectively targeting high-productivity entrepreneurs. This could potentially entail improvements in externally identifying entrepreneurs (see Fafchamps and Woodruff (2017), Hussam et al. (2017), and Mckenzie and Sansone (2017)). Alternatively, financial institutions could try to design better screening mechanisms for self-targeting (Beaman et al., 2014). From an academic perspective, more evidence regarding the extent to which different screening mechanisms or lending schemes prevent high-productivity households from obtaining credit is needed. For instance, using the context of the Million Baht program, Vera-Cossio (2018) shows that government-funded programs with a community-based approach can be vulnerable to resource capture leading to misallocation. In contrast, Beaman et al. (2014) show that, in absence of government intervention, interest rates may allow high-return households to self-select into credit. Yet, it is not clear how tools intended to screen low-risk clients such as peer referrals, group lending, scoring models, or alternative lending schemes such as self-help groups, promote or preclude the provision of credit to high-return clients.

3.8 Figures



(a) Program credit



(b) Total short-term credit

Figure 3.1. Effects of the program rollout on short-term credit

Note: The figure depicts flexible estimates corresponding to the specification in (3.5). Each dot represents differences in program borrowing between households from villages with high and low per-capita program funds, for each year with respect to the year of the announcement of the program (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per-capita TBH in each village on the corresponding outcome. High A: household belongs to the top-third of the baseline productivity distribution in each village. Low A: households belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control function approach using household beliefs about profits as a proxy variable. 95 % confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design.

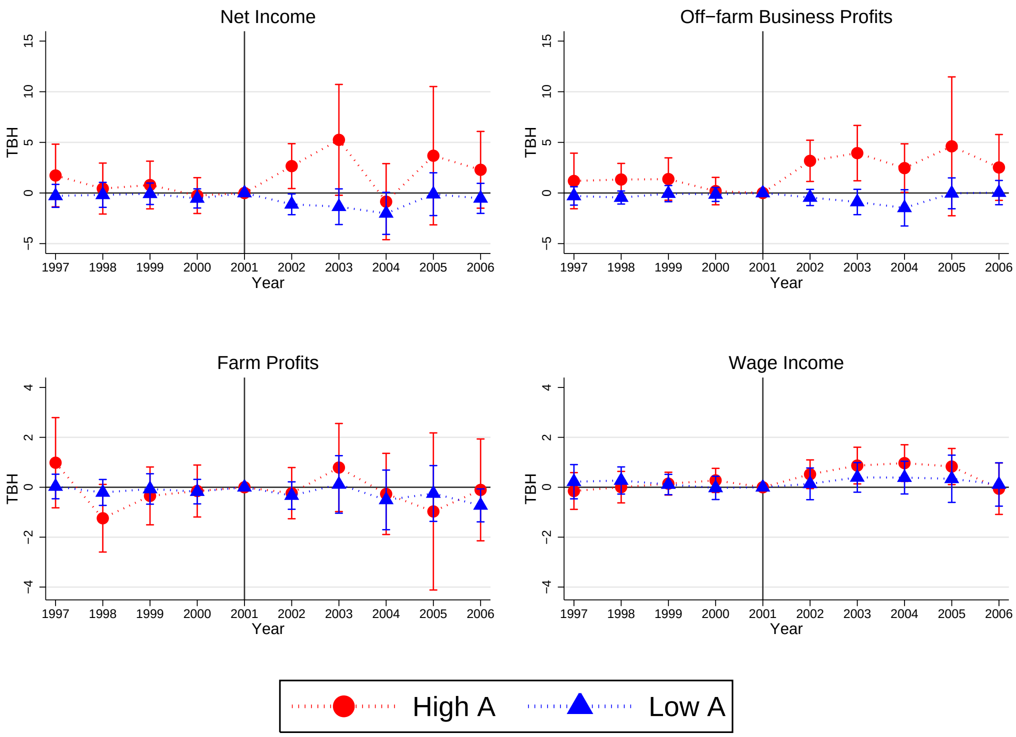


Figure 3.2. Reduced-form effects on household income - Proxy-variable approach

Note: The figure depicts flexible estimates corresponding to the specification in (3.5).Each dot represents differences in income between households from villages with high and low per-capita program funds, for each year with respect to the year of the announcement of the program (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per-capita TBH in each village on the corresponding outcome. High A: household belongs to the top-third of the baseline productivity distribution in each village. Low A: households belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control function approach using household beliefs about profits as a proxy variable. 95 % confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design.

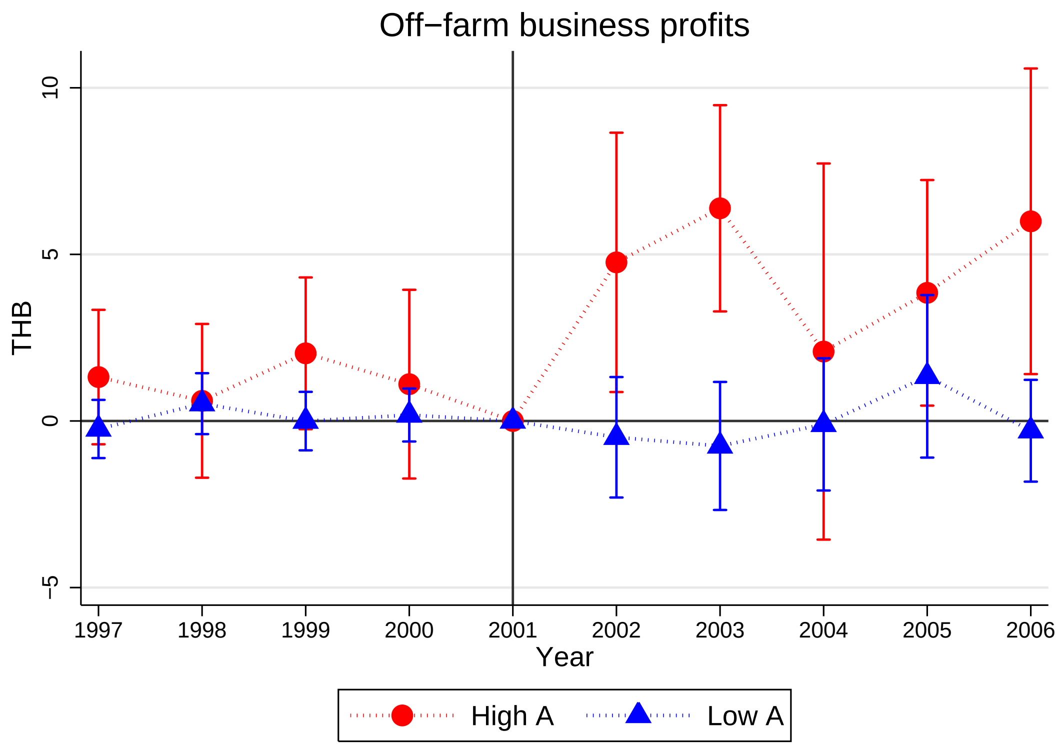


Figure 3.3. Reduced-form effects on business profits - Business owners only

Note: The figure depicts flexible estimates corresponding to the specification in (3.5).Each dot represents differences in program borrowing between households from villages with high and low per-capita program funds, for each year with respect to the year of the announcement of the program (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per-capita TBH in each village on the corresponding outcome. The effects are estimated over a sample of 230 households who reported holding business assets during the year preceding the rollout of the program. High A: household belongs to the top-third of the baseline productivity distribution in each village. Low A: households belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control function approach using household beliefs about profits as a proxy variable. 95 % confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design. The dependent variable is winsorized with respect to the top and bottom 1% of the distribution.

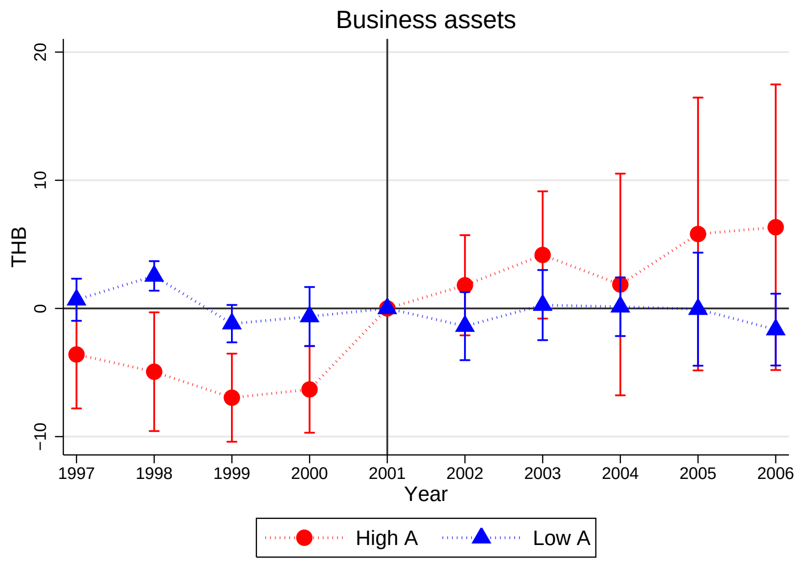


Figure 3.4. Reduced-form effects on business assets for owners of pre-existing businesses

Note: The figure depicts flexible estimates corresponding to the specification in (3.5). Each dot represents differences in program borrowing between households from villages with high and low per-capita program funds, for each year with respect to the year of the announcement of the program (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per-capita TBH in each village on the corresponding outcome. The effects are estimated over a sample of 230 households who reported holding business assets during the year preceding the rollout of the program. High A: household belongs to the top-third of the baseline productivity distribution in each village. Low A: households belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control function approach using household beliefs about profits as a proxy variable. 95 % confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design. The dependent variable is winsorized with respect to the top and bottom 1% of the distribution.

3.9 Tables

Table 3.1. Baseline Summary Statistics

Variable	N	Mean	SD
Household head is a male	4423	0.74	0.44
Age (household head)	4423	52.85	13.43
Years of schooling (household head)	4343	6.04	3.18
Number of household members	4603	4.44	2.06
Farm (share of net operating income)	4291	0.5	2.29
Fish/shrimp (share of net operating income)	4291	-0.03	2.4
Off-farm business (share of net operating income)	4291	0.1	0.82
Wage income (share of net operating income)	4291	0.43	1.88
Number of household off-farm businesses	4423	0.35	0.55
Household opened a new business (past 12 months)	4603	0.04	0.2
Net per-capita income (TBH)	4423	21299.6	34592.26
Per-capita consumption spending (TBH)	4423	12046.87	35602.31
Household borrows (institution or informal)	4603	0.78	0.41
Household borrows from formal/quasi-formal sources of credit	4603	0.56	0.5
Household borrows from informal sources of credit	4603	0.49	0.5

Note: The table presents summary statistics corresponding to the study sample and survey waves preceding the program (1997-2001). Farm activities includes cultivation of several crops as well as livestock produce. Institutional credit includes credit from commercial banks, BAAC (the state-owned bank) and other quasi-formal sources of credit such as cooperatives, and village-credit groups.

Table 3.2. Estimates of Value-added Production Functions

	(1)	(2)	(3)
	Log Value Added		
	OLS	FE	Control function
β_k	0.360***	0.015	0.380***
	(0.025)	(0.036)	(0.050)
Observations	2,622	2,622	2,622
R-squared	0.137	0.004	
# of Households	835	835	835
Persistance ω			0.238***
			(0.032)
Mean ω	5.15	9.67	4.98
SD ω	1.02	1.17	0.06

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents estimates of the elasticity of value-added with respect to capital β_k obtained using the 5 survey waves preceding the program (1997-2001). Column (1) presents OLS estimates for reference, columns (2)-(3) present estimates computed through the fixed-effects approach and the control-function approach, respectively. The bottom panel presents summary statistics for the estimates of log-productivity ($\omega = log(A)$). Standard errors corresponding to the control-function approach are computed using block bootstrap with 1000 iterations.

Table 3.3. Reduced-form Effects of the Program on Household Profits

Panel A: Proxy-variable approach

	(1) VF short-term Credit	(2) Short-term Credit	(3) Household Income	(4) Wage Income	(5) Profits	(6) Farm Profits	(7) Shrimp/Fish Profits	(8) Business Profits
Post X Inv HH X High Productivity	0.0314	0.643	3.309**	0.471	3.286**	0.579	-0.109	2.816**
	(0.106)	(0.383)	(1.642)	(0.393)	(1.274)	(0.437)	(0.133)	(1.225)
	[0.077]	[0.473]	[1.621]	[0.451]	[1.503]	[0.457]	[0.179]	[1.426]
Post X Inv HH	0.612***	0.781**	-0.830	0.106	-0.601	-0.227	-0.0391	-0.335
	(0.124)	(0.282)	(0.541)	(0.209)	(0.465)	(0.287)	(0.0845)	(0.431)
	[0.110]	[0.366]	[0.586]	[0.230]	[0.560]	[0.326]	[0.090]	[0.476]
Effect for High Productivity	0.644***	1.424**	2.478*	0.577	2.685*	0.352	-0.148	2.481*
SE	0.0671	0.485	1.421	0.329	1.301	0.459	0.189	1.122
SE (bootstrap)	[0.079]	[0.617]	[1.439]	[0.369]	[1.503]	[0.519]	[0.255]	[1.299]
Observations	8659	8659	8659	8659	8659	8659	8659	8659
Number of households	922	922	922	922	922	922	922	922
R-Squared	0.586	0.383	0.516	0.729	0.479	0.252	0.301	0.425

Panel B: Fixed-effects approach

	(1) VF short-term Credit	(2) Short-term Credit	(3) Household Income	(4) Wage Income	(5) Profits	(6) Farm Profits	(7) Shrimp/Fish Profits	(8) Business Profits
Post X Inv HH X High Productivity	-0.239***	0.0243	3.282*	0.568	2.678*	0.328	-0.172	2.521
	(0.0697)	(0.509)	(1.782)	(0.381)	(1.528)	(0.607)	(0.205)	(1.528)
Post X Inv HH	0.696***	0.983***	-0.555	0.114	-0.149	-0.0676	-0.0147	-0.0672
	(0.0827)	(0.228)	(0.426)	(0.227)	(0.382)	(0.256)	(0.0691)	(0.341)
Effect for High Productivity	0.457	1.007	2.727	0.682	2.529	0.261	-0.186	2.454
SE	0.101	0.624	1.683	0.300	1.594	0.641	0.238	1.462
Baseline mean (DV)	31.62	16596.5	98845.5	33458.4	41844.6	25414.5	1114.5	15315.7
Observations	8659	8659	8659	8659	8659	8659	8659	8659
Number of households	922	922	922	922	922	922	922	922
R-Squared	0.588	0.387	0.516	0.729	0.479	0.252	0.302	0.426

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table reports the reduced-form estimates of the effects of the program as a function of productivity estimated through the proxy-variable approach (Panel A) and the fixed-effects approach (Panel B). The coefficients have been scaled down by 1,000,000 in order to capture the effect of an additional per-capita TBH in each village on the corresponding outcome. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Panel A also presents bootstrap standard errors in brackets, which are computed using 500 bootstrap samples and clustered at the village level. Short-term credit involves loans with a term shorter than a year. Household profits include farm, fishing and shrimping and off-farm business profits. Farm profits include profits from agriculture and livestock.

Table 3.4. Reduced-form Effects of the Program on Off-farm Business Activities

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Proxy-variable approach						
	VF short-term credit	Total short-term credit	Profits	Non-wage Expenses	Wage Expenses	Assets
Post X Inv HH X High Productivity	-0.00998	2.148*	11.21**	1.138	0.480	16.35*
	(0.167)	(0.990)	(4.401)	(5.938)	(0.352)	(8.814)
	[0.165]	[1.135]	[5.069]	[5.506]	[0.365]	[8.972]
Post X Inv HH	0.577***	0.606	-0.487	-3.751	-0.0840	-0.304
	(0.194)	(0.516)	(1.403)	(2.424)	(0.120)	(1.144)
	[0.220]	[0.689]	[2.267]	[2.654]	[0.226]	[2.655]
Effect-High Productivity	0.567***	2.754**	10.72**	-2.613	0.396	16.04*
SE	0.135	1.154	3.937	5.335	0.368	9.403
SE (bootstrap)	[0.171]	[1.207]	[4.133]	[5.111]	[0.364]	[9.008]
Baseline mean (DV)	11.75	21237.0	50973.5	109802.4	7291.8	119203.8
Observations	2190	2190	2190	2190	2190	2190
Number of households	229	229	229	229	229	229
R-Squared	0.590	0.509	0.439	0.468	0.470	0.589
Panel B: Fixed-effects approach						
	(1)	(2)	(3)	(4)	(5)	(6)
	VF short-term credit	Total short-term credit	Profits	Non-wage Expenses	Wage Expenses	Assets
Post X Inv HH X High Productivity	-0.434***	1.211	11.61***	-3.524	0.409	13.52
	(0.145)	(1.152)	(3.689)	(6.485)	(0.490)	(9.362)
Post X Inv HH	0.719***	1.012***	-0.159	-2.055	-0.0306	1.161
	(0.144)	(0.364)	(1.103)	(1.343)	(0.108)	(2.308)
Effect High Productivity	0.285	2.223	11.45	-5.578	0.379	14.68
SE	0.231	1.394	3.812	6.237	0.462	9.600
Baseline mean (DV)	11.75	21237.0	50973.5	109802.4	7291.8	119203.8
Observations	2190	2190	2190	2190	2190	2190
Number of households	229	229	229	229	229	229
R-Squared	0.592	0.511	0.439	0.469	0.471	0.588

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table reports the reduced-form estimates of the effects of the program as a function of productivity estimated through the proxy-variable approach (Panel A) and the fixed-effects approach (Panel B). The coefficients have been scaled down by 1,000,000 in order to capture the effect of an additional per-capita TBH in each village on the corresponding outcome. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Panel A also presents bootstrap standard errors in brackets, which are computed using 500 bootstrap samples and clustered at the village level. The estimating sample includes only household who reported owning business assets the year preceding the rollout of the program.

Table 3.5. IV Estimates of the Effects of Total Credit on Income and Profits

Panel A: IV estimates of the effect of credit on household income (full sample)

	(1)	(2)	(3)	(4)	(5)	(6)
	Household Income		Wage Income		Total Profits	
	Raw	Winsorized	Raw	Winsorized	Raw	Winsorized
Total Short Term Credit * High Productivity	2.292*	1.658**	0.341	0.314	2.535**	1.211***
	(1.345)	(0.839)	(0.399)	(0.380)	(1.022)	(0.399)
	[1.239]	[0.727]	[0.404]	[0.406]	[1.062]	[0.580]
Total Short Term Credit	-0.932	-0.978**	0.147	0.123	-0.778	-0.825***
	(0.625)	(0.476)	(0.279)	(0.273)	(0.503)	(0.308)
	[0.879]	[0.564]	[0.357]	[0.352]	[0.738]	[0.416]
Effect- High Productivity	1.361*	0.681	0.488**	0.436	1.757**	0.386
SE	1.060	0.616	0.243	0.211	0.936	0.352
SE bootstrap	[0.778]	[0.455]	[0.245]	[0.226]	[0.777]	[0.423]

Panel B: IV estimates of the effect of credit on Profits by source (full sample)

	(1)	(2)	(3)	(4)	(5)	(6)
	Farm		Fishing/Shrimping		Off-farm Business	
	Raw	Winsorized	Raw	Winsorized	Raw	Winsorized
Total Short Term Credit * High Productivity	0.471	0.418	-0.0849	0.0239	2.149*	0.769**
	(0.359)	(0.301)	(0.0955)	(0.0291)	(1.076)	(0.387)
	[0.499]	[0.394]	[0.097]	[0.060]	[1.125]	[0.519]
Total Short Term Credit	-0.332	-0.495**	-0.0291	-0.0153	-0.417	-0.315
	(0.322)	(0.245)	(0.0836)	(0.0285)	(0.501)	(0.323)
	[0.494]	[0.408]	[0.090]	[0.059]	[0.759]	[0.285]
Effect- High Productivity	0.139	-0.0766	-0.114	0.00860	1.732**	0.454
SE	0.309	0.244	0.147	0.00694	0.869	0.283
SE bootstrap	[0.334]	[0.208]	[0.151]	[0.013]	[0.679]	[0.373]
First-stage F-stat: Short Term Credit	5.403					
First-Stage F-stat: Interaction	5.126					
Observations	8650					
Number of households	914					

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table reports the instrumental-variables estimates of the effects of total short-term credit as a function of productivity estimated through the proxy-variable approach. Panel A presents effects on income by source and Panel B presents effects on profits by type of activity. Odd-numbered columns report IV coefficients after winsorizing the respective dependent variable with respect to the 1st and 99th percentile. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrap standard errors are presented in brackets, and are computed using 500 bootstrap samples and clustered at the village level. Short-term credit involves program loans with a term shorter than a year, and has been top coded with respect to the 99th percentile for precision. Household profits include farm, fishing and shrimping and off-farm business profits.

Table 3.6. IV Estimates of the Effect of Total Credit on Off-farm Businesses (Pre-existing Businesses Only)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Profits		Non-wage Expenses		Wage Expenses		Assets	
	Raw	Winsorized	Raw	Winsorized	Raw	Winsorized	Raw	Winsorized
Total Short Term Credit * High Productivity	6.331**	4.029***	3.648	1.239	0.38	0.152	7.009*	4.688***
	(2.263)	(1.187)	(3.044)	(2.209)	(0.196)	(0.0712)	(4.186)	(1.833)
	[2.847]	[1.579]	[3.377]	[2.364]	[0.279]	[0.103]	[3.858]	[1.744]
Total Short Term Credit	-2.174	-2.200**	-4.509	-1.012	-0.144	-0.0293	0.180	-0.941
	(1.857)	(1.062)	(3.199)	(1.817)	(0.203)	(0.0692)	(1.077)	(1.165)
	[2.534]	[1.391]	[3.054]	[1.699]	[0.292]	[0.094]	[2.106]	[1.566]
Effect- High Productivity	4.157***	1.830**	-0.861	0.227	0.236	0.123**	7.188**	3.747***
SE	1.502	0.559	2.250	1.777	0.146	0.0433	4.026	1.500
SE bootstrap	[1.326]	[0.802]	[2.537]	[1.764]	[0.175]	[0.057]	[3.117]	[0.991]
First-stage F-stat: Short-term credit	4.902							
First-Stage F-stat: Interaction	6.306							
Observations	2188							
Number of households	228							

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table reports instrumental-variables estimates of the effects of total short-term credit as a function of productivity estimated through the proxy-variable approach. Odd-numbered columns report IV coefficients after winsorizing the respective dependent variable with respect to the 1st and 99th percentile. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrap standard errors are also reported in brackets, and are computed using 500 bootstrap samples and clustered at the village level. Short-term credit involves program loans with a term shorter than a year. The estimating sample includes household with pre-existing businesses only.

3.10 Acknowledgements

Chapter 3, is currently being prepared for submission for publication of the material. Breza, Emily; Townsend, Robert; Vera Cossio, Diego ”Access to credit and productivity: evidence from Thai Villages”. The dissertation author has contributed significantly to the collaborative research.

Appendix A

Appendix for Chapter 1

A.1 Supplementary figures and tables

Table A.1. Distribution of Targeted Households by Alternative Criteria

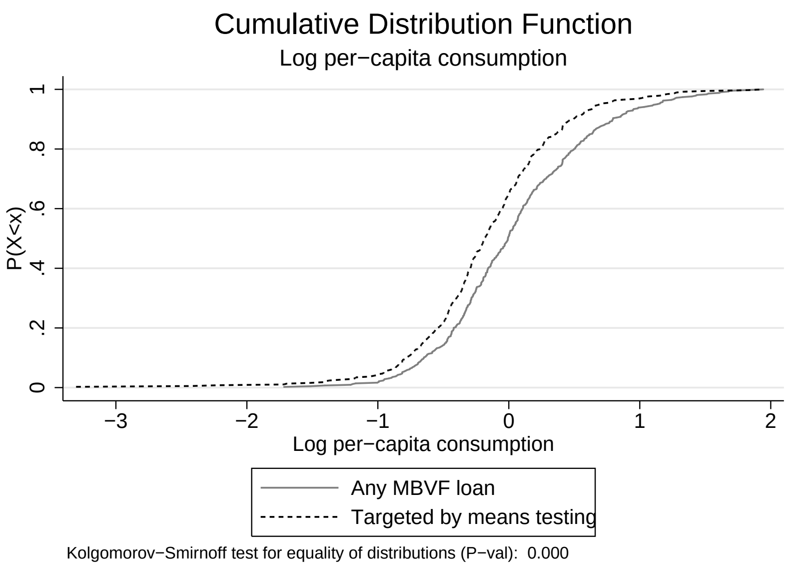
Panel A: Distribution of households under alternative allocation criteria

	Means testing	Credit score	Random assignment
Included in alternative only	24.08	21.97	19.48
Included in MBVF only	25.21	23.1	21.17
Included in both allocations	34.65	36.76	40.95
Excluded from both allocations	16.06	18.17	18.4

Panel B: Share of program beneficiaries which would have been excluded from the benchmark criteria

	Means testing	Credit score	Random assignment
Share	0.42	0.39	0.34

Note: The table presents the distribution of households across different targeting criteria. Each column represents an alternative targeting criteria–means testing, credit score, and random assignment. The first row in Panel A presents the share of households which would have been targeted by only the alternative targeting criterion but did not obtain credit from the program. The second row presents the share of households that obtained a loan from the program but would not have been eligible for a loan under the alternative criterion. The third row presents the share of households that obtained loans from the MBVF and would have also be eligible by the alternative criterion. The fourth row presents the share of households which would have been ineligible by the alternative criterion and did not borrow from the program. The reference period corresponds to the first two years following the implementation of the program. Panel B presents the share of program beneficiaries who would have been ineligible by alternative targeting criteria.



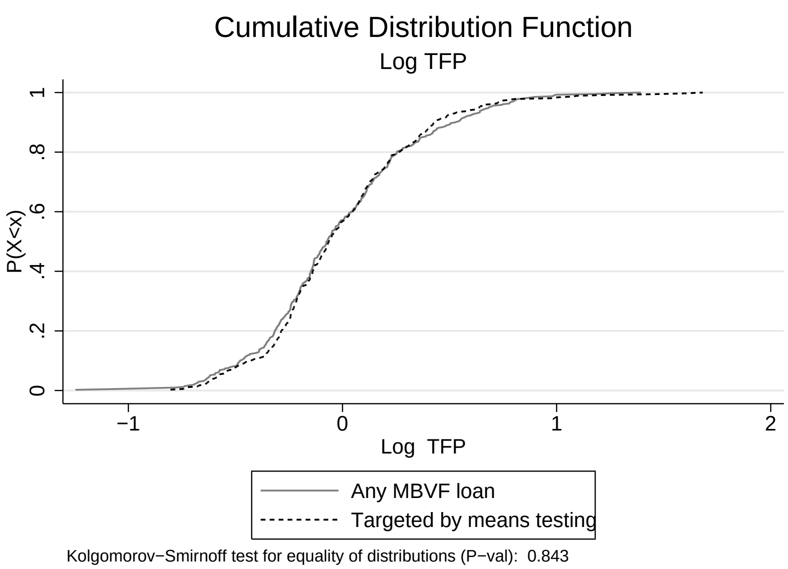


Figure A.1. Cumulative Distribution Functions of Baseline Consumption and Productivity by Different Criteria

Note: The top panel shows the cumulative density functions (CDF) of per-capita consumption (in logs) , measured at baseline, for households served by the program, and the baseline distribution of log per-capita consumption for households who would have been reached under the alternative criterion (MT). The bottom panel shows CDFs of value-added total factor productivity, measured at baseline, for households served by the program and for households who would have been reached under the alternative criterion. Both variables are centered with respect to the village mean in order to perform within-village comparisons. Per-capita consumption is measured as the total per-capita expenditure on consumption goods during the 12 months preceding the implementation of the program. Baseline total factor productivity is estimated using capital and labor elasticities corresponding to a value-added production function estimated as in Ackerberg et al. (2015).

Table A.2. Connections and Baseline Borrower Characteristics

VARIABLES	(1) Access to institutional credit	(2) Avg. delinquency rate	(3) Ever missed a payment	(4) Income volatility
Connected	0.155***	-0.003	0.133***	0.308***
	(0.036)	(0.007)	(0.040)	(0.084)
Constant	0.478***	0.027***	0.264***	0.591***
	(0.028)	(0.006)	(0.031)	(0.068)
Observations	649	616	616	649
R-squared	0.325	0.052	0.169	0.122

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents within-village comparisons of baseline characteristics across elite members or households directly connected with local elites and unconnected households. The table presents OLS coefficients from cross-section regressions of each baseline characteristic (columns) on an indicator that captures whether the household includes a village council member, a first-degree kin of council members or a member with pre-program socioeconomic interactions with village council member (Connected), after controlling for village fixed effects. Access to institutional credit is an indicator of whether a household held any loan from either formal lenders or quasi-formal lenders. The delinquency rate is computed as the share of loans for which the household had made any delinquent payments and is computed using repayment information for loans from all lender types, including loans from relatives and informal lenders. Income volatility: log of the coefficient of variation of monthly income computed over all the survey waves preceding the program. Robust standard errors are reported in parentheses.

Table A.3. Connections with Local Elites and Indicators of Poverty and Productivity

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log per-capita consumption (N=660)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Connected	0.058	0.124*	0.005	0.057	0.152**	-0.029
	(0.057)	(0.067)	(0.055)	(0.050)	(0.070)	(0.076)
Panel B: Total factor productivity (logs) (N=637)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Connected	0.003	0.114**	0.084**	0.032	-0.102*	-0.173***
	(0.047)	(0.050)	(0.042)	(0.047)	(0.056)	(0.051)
Panel C: Asset turnover (log revenues/assets) (N=680)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Connected	0.007	0.021***	0.018**	0.022	0.008	-0.007
	(0.040)	(0.005)	(0.008)	(0.015)	(0.052)	(0.151)
Panel D: Profitability margin (profits/revenues) (N=684)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Connected	-0.045	-0.111	-0.032	-0.100***	-0.044***	-0.021**
	(0.033)	(0.068)	(0.034)	(0.019)	(0.010)	(0.010)
Panel E: Total factor productivity (logs) -Revenue function dynamic panel (N=637)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Connected	-0.244***	-0.002	-0.077	-0.194***	-0.238***	-0.317***
	(0.072)	(0.073)	(0.059)	(0.058)	(0.080)	(0.120)

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: Column (1) presents coefficients corresponding to a regression of baseline characteristics on an indicator of won an indicator that captures whether the household includes a village council member, a first-degree kin of council members or a member with pre-program socioeconomic interactions with village council member (Connected). Columns (2)-(6) present results for equivalent quantile regressions. The bandwidth used for the estimation of quantile regressions was selected using Hall-Sheather's method. Robust standard errors are presented in parentheses. Panel A reports results for baseline per-capita consumption (in logs). Baseline per-capita consumption is measured as total expenditures during the 12 months preceding the implementation of the program. Panel B reports results for baseline log total factor productivity estimates recovered using capital and labor elasticities corresponding to a value-added production function estimated as in Ackerberg et al. (2015). Panel C presents results for baseline asset turnover ratio (in logs) computed as the average ratio of total revenues over a calendar year divided by the average stock of fixed assets in each household, over the two calendar years preceding the program's rollout (1999-2000). Panel D presents estimates for baseline profitability margins measured as the average ratio of net revenues (net of costs of purchased inputs outside the household) to gross revenues in a given year. Panel E presents results for baseline log total factor productivity estimates recovered using capital, labor, and intermediate inputs elasticities corresponding to a gross-revenue function estimated using a dynamic panel estimator.

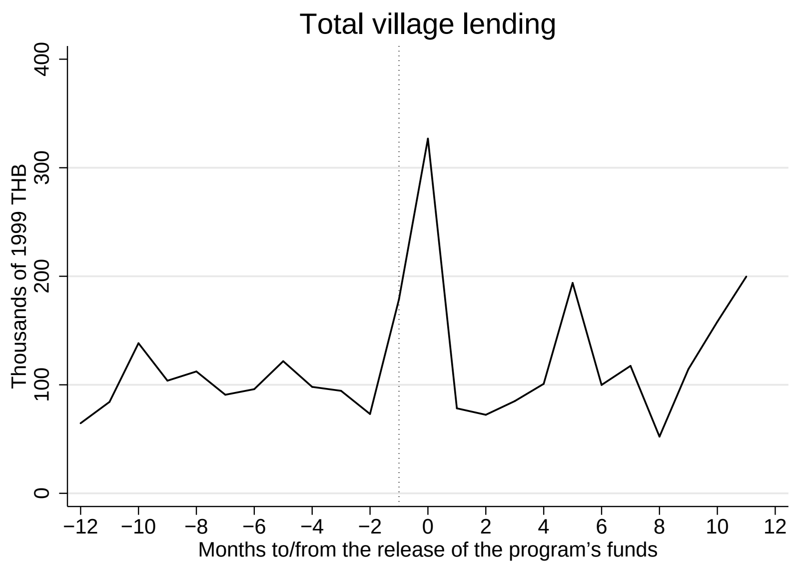


Figure A.2. Average Village Lending

Note: The top panel shows depicts village means for total lending in the months around the program rollout. The dotted line denotes the month preceding the release of the program’s funds.

Table A.4. Effects on Program and Total Borrowing

	Panel A: Effects on credit from the program					
	Any credit from MBVF			**Gross debt from MBVF**		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	Connected	Unconnected	All	Connected	Unconnected
$Post_{vt}$	0.328***	0.384***	0.233***	5,529.391***	7,092.555***	2,538.676***
	(0.019)	(0.026)	(0.029)	(373.757)	(527.504)	(409.526)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.008]
Observations	23,228	14,830	8,398	23,155	14,779	8,376
R-squared	0.613	0.632	0.564	0.590	0.619	0.523
Clusters (# households)	671	430	241	671	430	241

	Panel B: Effects on total credit					
	Any credit			**Total Gross outstanding debt**		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	Connected	Unconnected	All	Connected	Unconnected
$Post_{vt}$	0.074***	0.070***	0.086***	3,264.857*	4,689.658*	601.350
	(0.013)	(0.014)	(0.024)	(1,965.708)	(2,642.890)	(2,863.731)
	[0.000]	[0.004]	[0.008]	[0.120]	[0.124]	[0.856]
Observations	23,228	14,830	8,398	23,128	14,795	8,333
R-squared	0.661	0.628	0.660	0.866	0.825	0.910
Baseline DV mean	0.665	0.747	0.521	60747	59840	62356
Clusters (# households)	671	430	241	671	430	241

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on total borrowing, by connectedness with the local elite. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (2.2)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Panel A reports results for the effect of the rollout of the program on the program's uptake and Panel B shows results for total borrowing (winsorizing the top 1% of observations). Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

Table A.5. Short-run Effects on Credit from Non-program Institutional Lenders

Panel A: Effects on any credit from non-program institutional lenders

	BAAC			Local credit groups		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	Connected	Unconnected	All	Connected	Unconnected
$Post_{vt}$	0.015*	0.014	0.020	0.015	0.024	0.010
	(0.008)	(0.010)	(0.013)	(0.013)	(0.017)	(0.018)
	[0.092]	[0.184]	[0.120]	[0.412]	[0.352]	[0.668]
Observations	23,228	14,830	8,398	23,228	14,830	8,398
R-squared	0.842	0.830	0.852	0.666	0.661	0.643
Baseline DV mean	0.366	0.434	0.247	0.255	0.313	0.152
Clusters (# households)	671	430	241	671	430	241

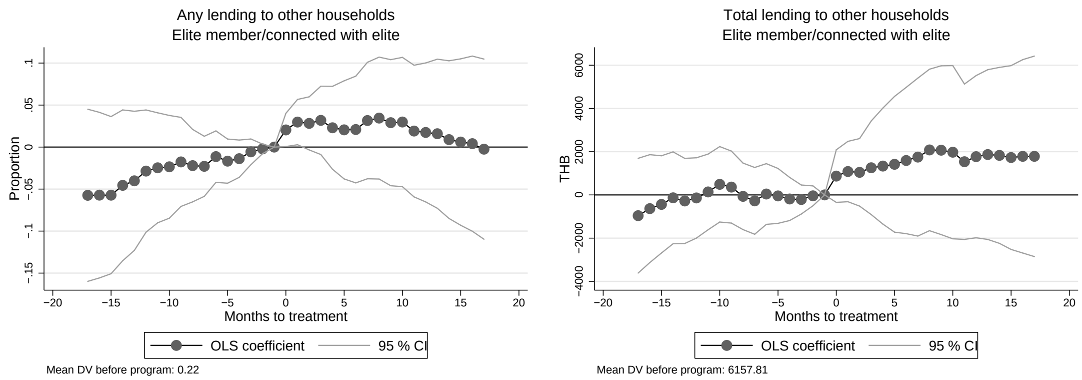
Panel B: Effects on total credit from non-program instutional lenders

	BAAC			local credit groups		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	Connected	Unconnected	All	Connected	Unconnected
$Post_{vt}$	602.755	552.604	1,018.859	114.126	97.648	350.761
	(1,074.413)	(1,728.284)	(1,069.061)	(660.160)	(852.427)	(1,075.008)
	[0.392]	[0.620]	[0.324]	[0.544]	[0.544]	[0.632]
Observations	23,095	14,747	8,348	23,106	14,747	8,359
R-squared	0.876	0.857	0.914	0.773	0.796	0.720
Baseline DV mean	23369	26650	17565	6890	8409	4204
Clusters (# households)	670	430	240	671	430	241

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on borrowing from non-program institutional lenders, by connectedness with the local elite. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (2.2)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

Figure A.3. Short-term Effects on Lending to Other Households



Note: The figure depicts OLS point estimates from a flexible difference-in-difference model following equation 1.6. The left panel presents estimates for the probability of lending to other households, and the right panel presents estimates for total lending to other households. Each dependent variable was regressed on household fixed effects, calendar month and year fixed effects, and a set of indicators that denote time to treatment. Each dot represents the coefficient associated with each of these indicators. The base category corresponds to the period preceding the first month of operation of the fund: $\tau_{vt} = -1$. Confidence intervals are constructed using standard errors clustered at the household level, to account for serial correlation. The estimation sample includes only households with baseline connections with the local elites.

Table A.6. Short-run Effects on Lending to Other Households

	Connected		Unconnected	
	(1)	(2)	(3)	(4)
VARIABLES	Any lending	Total lending	Any lending	Total lending
Post	0.033**	730.314	0.011	398.918
	(0.014)	(917.580)	(0.016)	(431.449)
	[0.016]	[0.548]	[0.424]	[0.432]
Observations	13,212	13,097	6,948	6,879
R-squared	0.783	0.862	0.783	0.675
Baseline DV mean	0.207	6148	0.140	2798
Clusters (# households)	367	365	193	193

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on lending to other households, by connectedness with the local elite. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (2.2)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

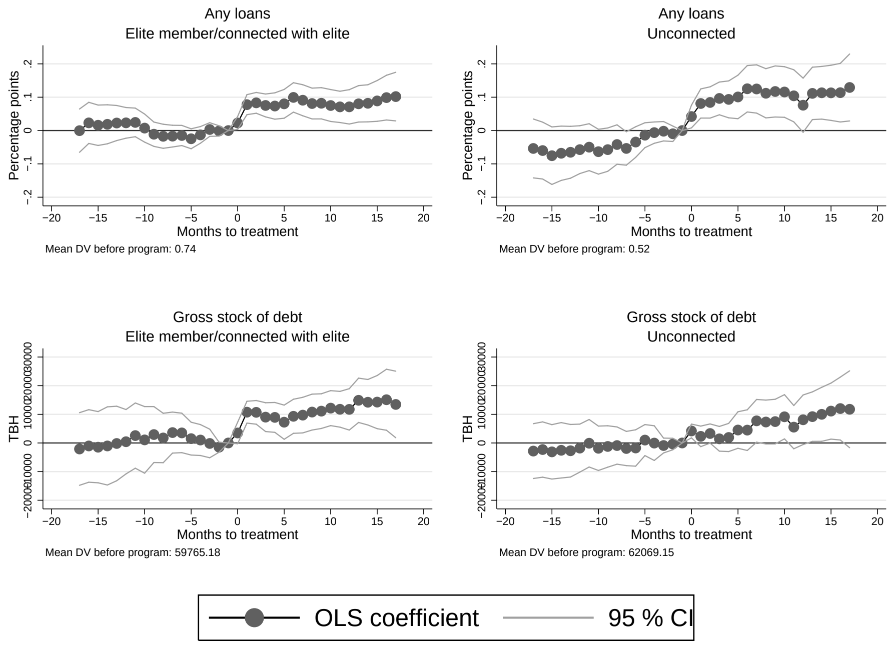


Figure A.4. Short-term effects on Total Borrowing

Note: The figure depicts OLS point estimates from a flexible difference-in-difference model following equation 1.6. The top panel reports coefficients for the probability of holding any outstanding loan from any source (both institutional and informal). The bottom panel presents results for the stock of outstanding debt. Results for connected households are shown in the left-hand panels while results for unconnected households are shown in the right-hand panels. Each dependent variable was regressed on household fixed effects, calendar month and year fixed effects, and a set of indicators that denote time to treatment. Each dot represents the coefficient associated with each of these indicators. The base category corresponds to the period preceding the first month of operation of the fund: $\tau_{vt} = -1$. Confidence intervals are constructed using standard errors clustered at the household level, to account for serial correlation.

Table A.7. Summary Statistics for Baseline Characteristics(1999-2000)

Summary statistics N=675

Panel A: Demographic characteristics

Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Household size	4.09	1.78	1.00	14.75
Males	1.94	1.11	0.00	8.00
Females	2.14	1.15	0.00	6.75
Mean hh age	35.59	13.78	12.15	89.88
Head of household is male	0.76	0.43	0.00	1.00
Mean hh years of schooling	4.27	2.39	0.00	16.00

Panel B: Land and wealth

Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Landless	0.22	0.42	0.00	1.00
Land in hectares	21.46	32.66	0.00	320.00
Land value/Assets	0.48	0.34	0.00	0.99
Total household assets	1826612	6393885	3463	143000000

Panel C: Revenues

Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Total household revenues	224866	630660	0	11900000
Cultivation (share)	0.34	0.35	0	1
Livestock (share)	0.08	0.21	0	1
Fishing-Shrimping (share)	0.06	0.18	0	1
Off-farm business (share)	0.11	0.26	0	1
Wage labor (share)	0.32	0.36	0	1
Other (share)	0.09	0.18	0	1
Cultivation (any)	0.74	0.44	0	1
Livestock (any)	0.65	0.48	0	1
Fishing-Shrimping (any)	0.41	0.49	0	1
Off-farm business (any)	0.31	0.46	0	1
Wage labor (any)	0.78	0.42	0	1
Other (any)	0.84	0.37	0	1
Number of sources of revenue	3.73	1.30	0	6

Panel D: Per-capita annual income and consumption (1999 TBH)

Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Per-capita income	21306	90105	0	2030435
Per-capita consumption	15060	13271	0	193597

Note: The table presents summary statistics for demographic and productive characteristics corresponding to the two years preceding the rollout of the MBVF program for the households in the Townsend-Thai Monthly Survey.

Table A.8. Summary Statistics for Credit Adoption by Type of Lender

Panel A: Full sample (N=643)					
Variable	**Mean**	**Median**	**Std. Dev.**	**Min**	**Max**
Any loans (any source)	0.67	1	0.47	0	1
Any formal/quasi-formal loans	0.58	1	0.49	0	1
Any informal loans	0.31	0	0.46	0	1
Number of loans (total)	1.76	1	2.14	0	18
Number of loans (formal+quasi-formal)	1.12	1	1.32	0	8
Number of loans (informal)	0.64	0	1.39	0	14
Gross stock of debt (total)	60747	20000	120655	0	1015000
Gross stock of debt (formal+quasi-formal)	50235	9500	110795	0	890000
Gross stock of debt (informal)	8076	0	21900	0	200000
Panel B: Village council members (elites) (N=60)					
Variable	**Mean**	**Median**	**Std. Dev.**	**Min**	**Max**
Any loans (any source)	0.85	1	0.36	0	1
Any formal/quasi-formal loans	0.82	1	0.38	0	1
Any informal loans	0.33	0	0.47	0	1
Number of loans (total)	2.82	2	2.66	0	17
Number of loans (formal+quasi-formal)	2.11	2	1.75	0	8
Number of loans (informal)	0.70	0	1.47	0	11
Gross stock of debt (total)	81791	39625	116003	0	762000
Gross stock of debt (formal+quasi-formal)	72502	30900	114488	0	762000
Gross stock of debt (informal)	9289	0	22731	0	172000
Panel C: Households with baseline connections with the elites (N=352)					
Variable	**Mean**	**Median**	**Std. Dev.**	**Min**	**Max**
Any loans (any source)	0.73	1	0.44	0	1
Any formal/quasi-formal loans	0.66	1	0.47	0	1
Any informal loans	0.34	0	0.47	0	1
Number of loans (total)	2.03	1	2.26	0	18
Number of loans (formal+quasi-formal)	1.29	1	1.32	0	8
Number of loans (informal)	0.74	0	1.54	0	14
Gross stock of debt (total)	56200	22000	104156	0	795400
Gross stock of debt (formal+quasi-formal)	46085	15000	96334	0	795400
Gross stock of debt (informal)	8477	0	21734	0	200000
Panel D: Households without baseline connections with the elites (N=231)					
Variable	**Mean**	**Median**	**Std. Dev.**	**Min**	**Max**
Any loans (any source)	0.52	1	0.50	0	1
Any formal/quasi-formal loans	0.39	0	0.49	0	1
Any informal loans	0.25	0	0.43	0	1
Number of loans (total)	1.09	1	1.53	0	10
Number of loans (formal+quasi-formal)	0.61	0	0.93	0	6
Number of loans (informal)	0.49	0	1.09	0	8
Gross stock of debt (total)	62356	2780	142614	0	1015000
Gross stock of debt (formal+quasi-formal)	50936	0	128394	0	890000
Gross stock of debt (informal)	7160	0	21908	0	200000

Note: The table presents summary statistics for the probability of holding a loan, the number of outstanding loans, and gross stock of debt in a given month, by type of lender. Formal loans include loans from the Bank of Agriculture and Agricultural Cooperatives or commercial banks. Quasi-formal loans include loans from cooperatives, production credit groups (PCGs), village funds and other village organizations. Informal loans include loans both from personal lenders and relatives inside or outside of the village. Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

Table A.9. Demographic Characteristics by Membership in the Village Council

	Village council members (Elites) (N=60)		Directly connected with elites (N=352)		Unconnected (N=231)	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Females	2.12	1.17	2.21	1.20	2.16	1.34
Males	2.43	1.25	1.95	1.11	1.78	1.13
Females 15 to 64	1.37	0.76	1.35	0.77	1.31	0.86
Males 15 to 64	1.52	0.81	1.15	0.77	1.11	0.92
Average years of schooling (household)	5.32	1.79	4.51	1.75	4.76	2.39
Average age (household)	34.53	12.67	36.18	13.82	39.23	15.40
Head of household is male	0.93	0.25	0.76	0.43	0.71	0.45
Owns an off-farm business	0.97	0.18	0.84	0.37	0.57	0.50
Land (in rai)	226	286	137	169	122	233
Per-capita wealth (TBH in 1999 values)	908636	3675508	384790	701427	595293	1608951

Note: The table presents summary statistics for baseline demographic characteristics by relationship with members of the village council. Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

Table A.10. Baseline Socioeconomic and Kinship Relationships with Village Council Members

Variable	Obs	Mean	Median	Std. Dev.	Min	Max
Village council member (elites)	643	0.09	0.00	0.29	0	1
Directly transacts with elites	643	0.55	1.00	0.50	0	1
Degree with the elites	643	1.32	1.00	1.55	0	8
Geodesic distance to elites (excludes singletons)	631	1.30	1.00	0.72	0	4
Closeness to the elite	643	0.48	0.50	0.20	0	1
First degree relative with the elites	643	0.13	0.00	0.34	0	1

Table A.11. Summary Statistics for Connections with the Elite

Type of transaction	Obs	Mean	Std. Dev.	Min	Max
Assets purchase	583	0.06	0.24	0	1
Assets sale	583	0.05	0.23	0	1
Contribution/Transfer	583	0.02	0.13	0	1
Gift reception	583	0.05	0.22	0	1
Lending	583	0.05	0.22	0	1
Borrowing	583	0.08	0.27	0	1
Paid employee	583	0.25	0.43	0	1
Employer	583	0.11	0.32	0	1
Provides unpaid labor	583	0.22	0.42	0	1
Receives unpaid labor	583	0.21	0.41	0	1
Input sale	583	0.10	0.30	0	1
Input reception	583	0.30	0.46	0	1
Output sale	583	0.13	0.34	0	1
Output purchase	583	0.19	0.39	0	1

Note: Input sale and reception include physical inputs as well as mentoring and advising. Socioeconomic interactions are based on data corresponding to the periods preceding the rollout of the program. Calculations exclude village council members.

Table A.12. Poverty and Productivity by Baseline Access to Credit and Alternative Targeting Criteria

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log per-capita consumption (N=660)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
High Baseline access to institutional credit	0.161***	0.143**	0.060	0.082	0.195***	0.164
	(0.060)	(0.064)	(0.057)	(0.061)	(0.057)	(0.116)
Panel B: Total factor productivity (logs) (N=637)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
High Baseline access to institutional credit	-0.067	-0.051*	-0.027	0.007	-0.087*	-0.147**
	(0.044)	(0.030)	(0.034)	(0.047)	(0.051)	(0.068)
Panel C: Log per-capita consumption (N=660)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Offered credit under means-testing criterion	-0.417***	-0.253***	-0.309***	-0.428***	-0.500***	-0.577***
	(0.051)	(0.067)	(0.054)	(0.050)	(0.060)	(0.075)
Panel D: Total factor productivity (logs) (N=637)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Offered credit under means-testing criterion	-0.017	0.182***	0.139***	0.017	-0.131***	-0.178***
	(0.039)	(0.029)	(0.031)	(0.049)	(0.047)	(0.066)
Panel E: Log per-capita consumption (N=660)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Offered credit based on credit score	0.096*	0.080	0.072	0.126***	0.204***	0.177**
	(0.053)	(0.062)	(0.051)	(0.049)	(0.058)	(0.082)
Panel F: Total factor productivity (logs) (N=637)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Offered credit based on credit score	0.069*	0.020	0.059*	0.074*	0.119***	0.077
	(0.038)	(0.034)	(0.033)	(0.041)	(0.043)	(0.064)

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: Column (1) presents coefficients corresponding to a regression of baseline characteristics on an indicator of whether a household obtained institutional credit during the baseline periods (Panels A and B), an indicator of whether a household would have been offered credit under a counterfactual means-testing criterion(Panels C and D), and an indicator of whether a household would have been offered credit under a counterfactual allocation based on predicted credit scores (Panels E and F). Columns (2)-(6) present results for equivalent quantile regressions. The bandwidth use for the estimation of quantile regressions was selected using Hall-Sheather's method. Robust standard errors are presented in parentheses.

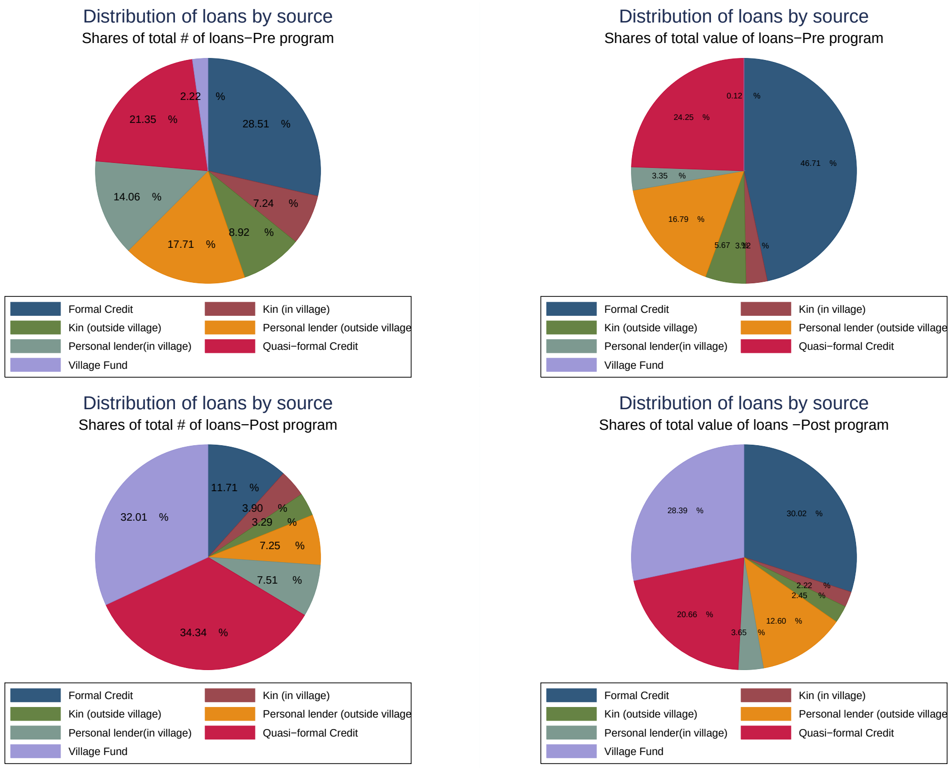


Figure A.5. Loan Portfolio Before and After the Program

Note: The top panel illustrates the distribution of loans by source (number and value of loans) for loans started between 1999 and 2000 (baseline periods). The bottom panel replicates the results for the two years following the rollout of the program. Formal loans include loans from the Bank for Agriculture and Agricultural Cooperatives (BAAC) and commercial banks. Quasi-formal loans include agricultural cooperatives and production credit groups (PCGs).

.

A.2 Productivity

Estimating value-added productivity

This section provides a detailed explanation of the estimation of total factor productivity from a value-added production function, following the approach proposed by Ackerberg et al. (2015). Value added (VA) is computed as total revenues R net of the value of the intermediate inputs M used to generate them over a calendar year. Assuming that households choose the amount of labor L and capital K to be used in order to generate value added, it is possible to represent the log value-added production function as follows (variables in lower case denote logs):

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_k k_{it} + \omega_{it} + \varepsilon_{it} \tag{A.1}$$

This expression is consistent with a Cobb-Douglas value-added production function, or a production function which is Leontief in intermediate inputs and Cobb-Douglas in capital and labor. This specification allows for the existence of two different shocks to production: shocks to productivity that are observed or forecasted by each household (ω_{it}) but not observed by the researcher, and shocks to production that are unobserved by both the household and the researcher(ε_{it}). As profit-maximizing households allocate capital and labor such that the marginal product of each factor equals the factor's price. This behavior leads to the main empirical challenge in the estimation of a the production function: Capital and labor are chosen based on the observed productivity shocks ω, which means that an OLS regression of log value added on log labor and log capital would be biased. Following the insights discussed in Olley and Pakes (1996) and Levinsohn and Petrin (2003), Ackerberg et al. (2015) propose a two-stage approach to recover consistent estimates of β_l and β_k as well as predicted values for the productivity shocks.

A.2.1 Identification assumptions

The identification of the parameters from equation (A.1) is achieved through assumptions corresponding to the information available to each household when deciding on the use of labor and capital, the process through which productivity evolves over time, and the extent to which input decisions can be adjusted in response to productivity shocks. This section intuitively describes these assumptions and refers the reader to Ackerberg et al. (2015) for more formal statements of these assumptions.

The first assumption is related to the information available to households at each point in time. The estimation approach assumes that during period t, households are aware of current productivity shocks as well as past productivity shocks; however, future shocks to productivity are not known by the households. Denote each household's information set at t as I_{it}. Since households do not expect or observe current transitory shocks to production ε_{it}, this assumption implies that the shocks to production are orthogonal to productivity shocks:

$$\mathbf{E}[\varepsilon_{it}|I_{it}] = 0 \tag{A.2}$$

The second assumption is related to the ability of households to use information to predict shocks to productivity and the persistence of these shocks. This paper assumes that productivity evolves according to a first-order Markov process, which is known to households:

$$\begin{aligned} \omega_{it} &= g(\omega_{it-1}) + \zeta_{it} \\ \mathbf{E}[\zeta_{it}|I_{it-1}] &= 0 \end{aligned} \tag{A.3}$$

This assumption, while restrictive in terms of the dynamics of productivity, is weaker than assumptions that would be made in an OLS approach or fixed-effects model or a dynamic panel approach (see for example Anderson and Hsiao (1982)). In the context of this study, it allows productivity at baseline to be a good predictor of productivity in the periods following the implementation of the program, and hence to be a relevant margin for evaluating the targeting performance of the program. The third assumption is related to the law of motion for the stock of capital (k_{it}). In particular, the assumption is that capital in the current period k_t is a function of the stock of capital and investment in the previous period k_{t-1}, i_{t-1}:

$$k_t = k(i_t, k_{t-1}) \tag{A.4}$$

This assumption means that capital is fixed in the sense that households would experience high costs to adjust their choices of capital in response to current productivity shocks. A further assumption is that labor decisions are made in any time period up to period t. Thus, labor is allowed to adjust with respect to current productivity shocks. In this sense, labor is a free input in this model. While this assumption implies that lagged values of l could be used as instruments for current values of labor, the fact that capital is pre-determined is not enough to recover consistent estimates of β_l and β_k, as investment might be a function of observed productivity and hence k_t may be correlated with ω_t given that there is persistence in the productivity shocks. Thus, variation in productivity still needs to be controlled for. The final two assumptions allow the researcher to control for variation in productivity by imposing some structure on the way intermediate inputs relate to productivity. The key assumptions in this approach are that conditional on their labor and optimal capital decisions, as well as the observed shocks to productivity, in each period households demand intermediate inputs according to a monotonically increasing function of ω_{it}, conditional on labor and capital choices:

$$\begin{aligned} m_{it} &= f_t(k_{it}, l_{it}, \omega_{it}) \qquad \text{(A.5)} \\ w_{it} &= f_t^{-1}(m_{it}, k_{it}, l_{it}) \end{aligned}$$

This assumption allows for inversion of f and use of the conditional variation in m to control for the variation in productivity shocks that are not observed by the researcher; that is, it allows ω to be written as a function of the intermediate input m, capital k, and labor l. This assumption rules out models in which there are adjustment costs to intermediate inputs, or models in which there are shortages in the supply of these inputs. While restrictive, the latter assumption has the advantage that it is testable as discussed in Levinsohn and Petrin (2003).

Moment conditions

Using the assumptions in (A.2) to (A.6), it is possible to derive the two moment conditions that will allow the identification of β_l and β_k.

$$\mathbf{E}[\varepsilon_{it}|I_t] = \mathbf{E}[y_{it} - \Phi_t(m_{it}, k_{it}, l_{it})|I_{it}] = 0 \qquad \text{(A.6)}$$

$$\mathbf{E}[\zeta_{it} + \varepsilon_{it}|I_{it-1}] =$$

$$\mathbf{E}[y_{it} - \beta_0 - \beta_l l_{it} - \beta_k k_{it} - g(\Phi_{t-1}(m_{it-1}, k_{it-1}, l_{it-1}) - \beta_l l_{it-1} - \beta_k k_{t-1})|I_{it-1}] = 0 \qquad \text{(A.7)}$$

with $\Phi_t = \beta_0 + \beta_l l_{it} + \beta_k k_{it} + f_t^{-1}(m_{it,l_{it},k_{it}})$ The first moment condition results from plugging in (A.6) into (A.1), and combining it with (A.2). The Second moment condition exploits the assumption that productivity evolves according to a first-order Markov process as in (A.4). Note that none of the structural parameters can be identified only from the first equation, however it is possible to use these moment conditions to identify $\hat{\Phi}_t$, and plug in $\hat{\Phi}_{t-1}(m_{it-1}, l_{it-1}, k_{it-1})$ into the second equation (A.7). The resulting set of moment conditions after this process is:

$$\mathbf{E}\left[\left(y_{it}-\beta_0-\beta_l l_{it}-\beta_k k_{it}-g(\Phi_{t-1})-\beta_l l_{it-1}-\beta_k k_{t-1}\right)\right)\otimes\begin{pmatrix}1\\ l_{it-1}\\ k_{it}\\ \hat{\Phi}_{it-1}\end{pmatrix}\right]=0 \qquad \text{(A.8)}$$

The behavioral assumptions made in this sections are represented in this set of moment conditions. First, as capital is pre-determined, k_t is a function of investment at $t-1$ and thus $k_{it} \in I_{it-1}$. This means that capital is chosen prior to observing innovations in the productivity process ζ_{it}. However, this approach does not restrict the adjustment of labor and it is perfectly possible that a household will adjust labor given the innovations ζ_{it}. The only restriction in terms of the adjustment of labor decisions is that households cannot forecast ζ_{it}, and thus their past labor decisions are orthogonal with respect to current innovations to productivity. Finally, note that there is no extra variation coming from the intermediate input m in the latter set of moment conditions; the relevant variation was already used to recover $\hat{\Phi}_t$ from (A.6). This last observation prevents identification of a elasticity parameter for m, and hence the identification of a revenue function without assuming that the underlying technology is Leontief in intermediate inputs. [1]

Estimation and variable definition

The estimation approach to recovering β_l and β_k follows the simplification detailed in Appendix A.4 in Ackerberg et al. (2015). This process reduces the system in (A.8) to:

$$\mathbf{E}\left[\hat{\zeta}_{it}\otimes\begin{pmatrix}l_{t-1}\\ k_t\end{pmatrix}\right]=0 \qquad \text{(A.9)}$$

[1] Gandhi et al. (2016) discuss this issue extensively and develop an alternative approach which in principle allows to estimate a revenue function and relax these assumptions.

with $\hat{\zeta}_{it} = (\hat{\Phi}_t - \beta_l l_{it} - \beta_k k_{it}) - h(\hat{\Phi}_{t-1} - \beta_l l_{it-1} - \beta_k k_{it-1})$. h is an arbitrary function. The estimation is performed through the generalized method of moments (GMM) using k_{it} and l_{it-1} as instruments. To operationalize this process, value added y is computed as the total revenues, over a calendar year t, net of the value of the inputs purchased outside the household that were used to generate revenue during the period (m_{it}). The proxy variable is the total value of inputs, purchased outside the household, that were used for generating revenues (m_{it}). These inputs include fertilizer and seeds for agriculture, tools for fishing, transportation spending, appliances to be used in off-farm family businesses, and labor from outside the household. Labor is measured as the total hours per year of labor employed in households's revenue-generating activities. On average 85% is provided by household members; this includes hours spent on agriculture, fishing, caring for cattle, working at the off-farm family business, and working for wages outside the household. Capital is measured as the value of the total stock of fixed assets, and to be consistent with the assumptions regarding the timing of the inputs, it is measured in January of each calendar year. Section A.2.1 discusses robustness checks against alternative measures of labor and capital.

Estimation procedure

Value added function estimation

The elasticities from the household value added function and the estimates for productivity are recovered following the process detailed below.

1. Using the 14 years of data, the first-stage regression corresponding to the sample analog of (A.6) is estimated. The function f_t^{-1} that maps productivity ω_{it} into the demand for intermediate inputs is approximated using a third-order polynomial on m, k, and l. To allow f to vary with changes in the price of final output and inputs over time and across villages–but which are common to households within a village–village-year fixed effects

(δ_{vt}) are included in the first stage:

$$y_{it} = \sum_{h=0}^{h=3}\sum_{j=0}^{j=3}\sum_{n=0}^{n=3} \phi_{hjn} m_{it}^{h} l_{it}^{j} k_{it}^{n} + \delta_{vt} + e_{it}$$

2. $\hat{\Phi}_t$ is computed as $\hat{\Phi}_t = \sum_{h=0}^{h=3}\sum_{j=0}^{j=3}\sum_{n=0}^{n=3} \phi_{hjn} m_{it}^{h} l_{it}^{j} k_{it}^{n} + \delta_{vt}$.

3. Using candidate values for β_k and β_l, obtained from an OLS regression, $\hat{\omega}_{it}$ is computed as:

$$\hat{\omega}_{it} = \hat{\Phi}_t - \beta_l l_{it} - \beta_k k_{it}$$

4. Since productivity is assumed to follow a first-order Markov process, the following equation is estimated:

$$\hat{\omega}_{it}(\beta_l, \beta_k) = \sum_{n=1}^{n=3} \psi_n \hat{\omega}_{it-1}(\beta_l, \beta_k) + \delta_{vt} + \tilde{\zeta}_i$$

5. The resulting residuals $\hat{\zeta}_i(\beta_l, \beta_k)$ are used to construct the sample analog of (A.9), and $\hat{\beta}_l$ and $\hat{\beta}_k$ are estimated using GMM.

6. To account for the uncertainty in the estimation of the first stage, standard errors are computed using 500 non-parametric block bootstrap samples stratified at the village level. Additionally, p-values associated with percentile t-bootstrap tests for significance are reported in order to provide an asymptotic correction for a small sample estimation.

7. Value-added productivity is recovered using the GMM estimates $\hat{\beta}_k$ and $\hat{\beta}_l$:

$$\hat{\omega}_{it}^{*} = \hat{\Phi}_t - \hat{\beta}_l l_{it} - \hat{\beta}_k k_{it}$$

8. For the analysis in the paper I only focuss on estimates of productivity $\hat{\omega}_{it}^{*}$ corresponding

to the average over the baseline years 1999-2000.

9. I also report results using only data from 1999-2001 to estimate the elasticities (baseline data only). Results are robust to this approach.

Revenue Function estimation

An alternative way of recovering factor elasticities and productivity is to estimate a household revenue function following a dynamic panel model by "ρ-differencing" the equation below:

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_k k_{it} + \beta_m m_{it} + \omega_{it} + \varepsilon_{it} \tag{A.10}$$

and assuming that ω follows a first-order autoregressive process: $\omega_{it} = \rho \omega_{it-1} + \zeta_{it}$. In this case, the dependent variable y denotes total revenues for a household, and m (intermediate inputs) is also included in the revenue function.

The estimation process is detailed below:

1. First I subtract ρy_{t-1} from both sides of the equations.

$$\begin{aligned}(y_{it} - \beta_k k_{it} - \beta_l l_{it} - \beta_m m_{it}) &= \rho(y_{it-1} - \beta_k k_{it-1} - \beta_l l_{it-1} - \beta_m m_{it-1}) \\ &\quad + \zeta_{it} + \varepsilon_{it} - \rho \varepsilon_{it-1}\end{aligned}$$

2. Using candidate values for β_k, β_m, and β_l obtained from an OLS regression of (A.10), $\hat{\omega}_{it}$ is computed as:

$$\hat{\omega}_{it} = (y_{it} - \beta_k k_{it} - \beta_l l_{it} - \beta_m m_{it}) - \beta_0$$

3. Since productivity is assumed to follow a first-order autoregressive process, the following

equation is estimated:

$$\hat{\omega}_{it}(\beta_l, \beta_k, \beta_m) = \rho\,\hat{\omega}_{it-1}(\beta_l, \beta_k, \beta_m) + \delta_{vt} + \tilde{\zeta}_i$$

where δ_{vt} include a full set of village-year fixed effects.

4. The resulting residuals $\hat{\zeta}_i(\beta_l, \beta_k, \beta_m)$ are used to construct the sample analog of:

$$\mathbf{E}\left[(\omega_{it} - \rho\,\omega_{it-1}) \otimes \begin{pmatrix} 1 \\ l_{it-1} \\ k_{it-1} \\ m_{it-1} \end{pmatrix}\right] = 0 \tag{A.11}$$

and $\hat{\beta}_l$ and $\hat{\beta}_k$ are estimated using GMM.

5. Revenue productivity is recovered using the GMM estimates $\hat{\beta}_k$ and $\hat{\beta}_l$:

$$\hat{\omega}^*_{it} = y_{it} - \hat{\beta}_l l_{it} - \hat{\beta}_k k_{it} - \hat{\beta}_m$$

6. I use only pre-program values of $\hat{\omega}_{it}$ corresponding to an average of predicted productivity for 1999-2000.

Table A.13. Value Added Function Estimates

Panel A: Production function estimates (Value-added Cols (1)-(9) Revenues (10)-(11))

	OLS (1)	FE (2)	ACF (all years) (3)	ACF (M.E. in k) (4)	ACF(pre-program) (5)	ACF (balanced panel) (6)	ACF (OI) (7)	DP (all years) (8)	DP (pre-program) (9)	DP (all years) (10)	DP (pre-program) (11)
Labor (log)	0.514***	0.391***	0.724**	0.698**	0.645***	0.821	0.432***	0.695	0.683	0.219	0.491
S.E.	(0.010)	(0.022)	(0.350)	(0.390)	(0.301)	(1.038)	(0.031)	(0.449)	(0.482)	(0.371)	(0.493)
P-val (Bootstrap)			{0.03}	{0.01}	{0.00}	{0.28}	{0.00}				
Capital (log)	0.232***	0.0838**	0.233***	0.253***	0.163***	0.232***	0.177***	0.247	0.174	0.0834	0.0804
S.E.	(0.008)	(0.032)	(0.129)	(0.161)	(0.134)	(0.103)	(0.017)	(0.176)	(0.188)	(0.161)	(0.211)
P-val (Bootstrap)			{0.00}	{0.00}	{0.00}	{0.00}	{0.00}				
Intermediate inputs (log)										0.508***	0.342*
S.E.										(0.119)	(0.157)
Obs.	7226	7226	6438	6438	1106	5317	6372	6372	1096	6417	1102
Returns to scale (RTS)	0.747	0.475	0.958	0.951	0.808	1.053	0.610	0.943	0.857	0.81	0.914
Chi2 (constant RTS)	446.4	184.2	0.00885	0.00838	0.201	0.00224	127.3	0.00856	0.0465	1.735	0.373
P-Val (constant RTS)	0.000	0.000	0.714	0.927	0.5306	0.808	0.000	0.926	0.829	0.188	0.541
Test for OI restrictions (Jstat)							0.365				
Test for OI restrictions (Pval)							0.856				

Panel B: Summary Statistics for baseline productivity

	OLS (1)	FE (2)	ACF (all years) (3)	ACF (M.E. in k) (4)	ACF(pre-program) (5)	ACF (balanced panel) (6)	ACF (OI) (7)	DP (all years) (8)	DP (pre-program) (9)	DP (all years) (10)	DP (pre-program) (11)
Mean	4.10	6.92	2.51	2.46	4.02	1.79	5.45	2.54	3.58	3.58	3.89
Sd	0.94	1.02	0.58	0.58	0.60	0.61	0.65	0.94	0.93	0.92	0.87

$***p < 0.01, **p < 0.05, *p < 0.1$ Based on Bootstrap-t p-values for columns (3)-(6)

Note: The table presents estimates of a production function from different approaches as well as tests for the null of constant returns to scale. All estimations control for village × year fixed effects. In columns (1) to (5), the dependent variable is Value added from all the economic activities of the household. It is computed by subtracting the value of the intermediate inputs from the total revenues for each household. Revenues correspond to agriculture, livestock-raising and fishing, paid labor and family business activities. Labor is measured in hours/year across all activities and includes work performed by household members as well as by people outside the household. Capital is the value of each household's fixed assets, measured at the beginning of each year. All variables are in logs. Column (1) presents OLS estimates, Column (2) presents fixed-effects estimates. Columns (3)-(6) report GMM estimates using all of the observations from all available periods (benchmark), correcting for potential measurement error in capital (instrumenting by the first lag of log capital), the benchmark estimation using only from pre-program periods, and only the sample of households observed during all waves of the survey, respectively. The instruments for these specifications are the first lag of labor and capital measured at the beginning of each year. Column (7) presents estimates from an overidentified model that also includes the second lag of labor and first lag of capital as instruments. Column(8)-(9) presents estimates from a dynamic panel model estimated through GMM using lagged versions of capital and labor as instruments. Columns (10) and (11) present estimates for a gross revenue function based following a dynamic panel approach. Standard errors from the two-stage procedure are presented in parentheses. P-values using the empirical distribution of the t-statistic derived from 500 bootstrap samples (percentile-t bootstrap), to allow for small sample asymptotic correction, are reported in braces.

Alternative specifications and discussion

To avoid imposing restrictive assumptions regarding the use of credit and the interactions of all possible sources of income that households may have, this paper uses the benchmark specification that employs total revenues over all activities and total expenditures on intermediate inputs. Table A.14 presents robustness checks of the productivity estimates associated with different definitions of labor, capital, and revenues. Column (1) replicates the benchmark estimates for comparison. Column (2) presents estimates from a model that excludes hired labor. In this case β_l only captures the contribution of labor provided by household members. While labor from household members accounts on average for 85% of total labor, and the resulting coefficients are similar with respect to the benchmark specification, excluding hired labor reduces the observations as there are some households that rely exclusively on hired labor. Column (3) excludes household assets from the computation of capital. Household assets are mainly composed of the value of the dwelling in which households live and other appliances in the household. The resulting estimates are basically identical to the benchmark specification. Finally, Column (4) reports estimates that exclude revenues and expenses related to paid labor outside the household. The resulting estimates are smaller in the case of β_l with respect to the benchmark cases. Note however that excluding revenues from wage labor reduces the available observations, as some households may rely exclusively on this source of revenue.

Table A.14. Production Function Estimates Under Alternative Specifications

Panel A: GMM Estimates of the Value-Added function

	Benchmark Specification (1)	**Excluding hired labor (2)**	**Excluding household assets (3)**	**Excluding wage earnings (4)**
Labor (log)	0.724*	0.812	0.745	0.634***
	(0.350)	(0.949)	(0.396)	(0.0497)
Capital (log)	0.233*	0.254	0.210*	0.213
	(0.110)	(0.160)	(0.0997)	(0.136)
Obs	6438	6231	6438	5592
Returns to Scale	0.958	1.066	0.955	0.846
Chi2 (Test for constant RTS)	0.00885	0.952	3.828	35.44
Pval (Test for constant RTS)	0.925	0.329	0.0504	0.000

Panel B:Value-added productivity estimates

	Benchmark Specification (1)	**Excluding hired labor (2)**	**Excluding household assets (3)**	**Excluding wage earnings (4)**
Mean	2.51	1.69	2.68	3.87
SD	0.58	0.72	0.59	0.77

Panel C: GMM Estimates of the Value-Added function

	ACF Farm (1)	**ACF No Farm (2)**	**DP Farm (3)**	**DP No Farm (4)**
Labor (log)	1.010	1.085	0.749	0.734
	(2.012)	(234.9)	(0.605)	(0.550)
Capital (log)	0.145	0.249	0.141	0.189
	(0.207)	(29.21)	(0.226)	(0.220)
Obs	3517	2921	3474	2898
Returns to Scale	1.155	1.335	0.890	0.923
Chi2 (Test for constant RTS)	0.006	0.000	0.0179	0.0103
Pval (Test for constant RTS)	0.943	0.999	0.894	0.919

Panel D:Value-added productivity

	Benchmark Specification (1)	**Excluding hired labor (2)**	**Excluding household assets (3)**	**Excluding wage earnings (4)**
Mean	1.42	-0.55	3.50	3.00
SD	0.85	0.70	0.95	0.94

Note: Panel A presents estimates of production function from different specifications using the method proposed by Ackerberg et al. (2015). All estimations control for village $\times$ year fixed effects both in the first and second stage and are estimated using GMM. The dependent variable is Value-added from all the economic activities of the household. It is computed by subtracting the value of intermediate inputs from the total revenues for each household. Revenues correspond to agriculture, livestock-raising and fishing, paid labor, and family business activities. Labor is measured in hours/year across all activities and includes work performed by household members as well as by people outside the household. Capital is the value of each household's fixed assets, measured at the beginning of each year. All variables are in logs. Column (1) replicates the benchmark specification used in the paper. Column (2) presents estimates excluding hired labor from the estimations. Column (3) presents estimates from a model that excludes households' assets from the computation of capital and Column (4) presents estimates of value-added excluding earnings and costs from labor outside the household. Bootstrap standard errors are clustered at the household level to account for serial correlation, and are presented in parentheses. Panel B provides summary statistics for productivity measures that were estimated using each specification. Panel C presents estimates for households for whom farm activities (agriculture, livestock and fishing) were on average the main sources of income Column (1) and for whom non-farm activities were the main source of income Column (2) using the approach proposed by Ackerberg et al. (2015). Columns (3)-(4) replicate this estimations using a dynamic panel approach.

Testing the monotonicity assumption

The main identifying assumption in this context is the existence of a demand function that maps the demand of intermediate inputs m purchased outside the household to productivity in a strictly monotonic way. The empirical implication of this assumption is that the productivity estimates should exhibit a strictly monotonic relationship to the value of the intermediate inputs, conditional on labor, capital, and village-year fixed effects. Figure A.6 provides a graphical test for the strict monotonicity assumption. The y-axis plots residuals from a regression of the value of intermediate inputs m_{it} on a third-order polynomial of labor and capital and a full set of village-year dummies. The x-axis plots residuals of a similar regression in which the dependent variable corresponds to the value-added productivity estimates. The picture depicts a clear monotonic relation among these variables, validating the main identification assumption in this approach.

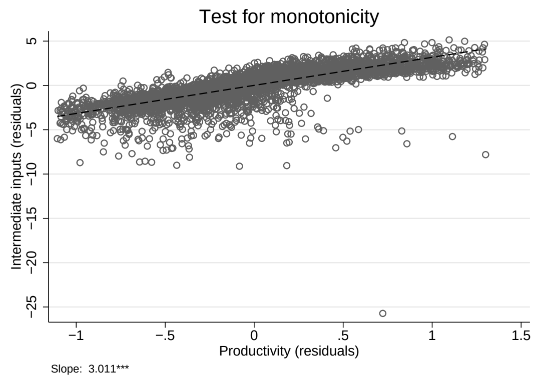


Figure A.6. Productivity and Intermediate Inputs

Note: The figure plots residuals of a regression of productivity on a third-degree polynomial of log labor and log capital, controlling for village-year fixed effects (x-axis) and residuals of a regression of log purchased inputs on a third-degree polynomial of log labor and log capital,controlling for village-year fixed effects (y-axis). Top and bottom 1% of observation are winsorized.

A more formal test for the validity of this assumption is provided by Shenoy (2017). The idea is that if firms were constrained with respect to the intermediate inputs or faced rigidities in the markets of intermediate inputs, production in period t should be a function of past input choices (first lags of capital, labor and intermediate inputs). I test for this using a two-stage approach. First, I regress log value-add y on a third order polynomial of current values of log capital,labor and intermediate inputs ($h(k_t, l_t, m_t)$), controlling for village-year fixed effects, and compute the residuals $\hat{\varepsilon_{it}}$. Then I regress these residuals on a vector $\mathbf{r}_{t-1}$ of lagged capital, labor and intermediate inputs and test the extent to which all the elements of the vector $\kappa = \mathbf{0}$:

$$\hat{\varepsilon}_i = \mathbf{r}_{t-1}\kappa + v_i \tag{A.12}$$

If households do not face constraints in the adjustment of inputs, then variation in output should be only explained by current choices of input and $\kappa = 0$. Table A.15 shows that the null of no constraints is not rejected under several specifications. While this validate the identification assumptions, note that this is not evidence of no credit constraints. For instance, households may hold excess on inventory simply because they don't have access to credit to finance increases in inputs when a households experiences positive productivity shocks.

Table A.15. Test for Frictions in Intermediate Inputs

Regressors ($\mathbf{r}_{t-1}$):	$k_{t-1}, l_{t-1}, m_{t-1}$	$\{k_{t-j}, l_{t-j}, m_{t-j}\}_{j=1}^{j=2}$	$\{k_{t-j}, l_{t-j}, m_{t-j}\}_{j=1}^{j=3}$	2nd order $f(k_{t-1}, l_{t-1}, m_{t-1})$	3rd order $f(k_{t-1}, l_{t-1}, m_{t-1})$
Observations	6,532	5,916	5,240	6,438	6,438
Adjusted R-squared	-0.000	-0.000	0.001	-0.000	0.003
F Stat :$\kappa = 0$	0.0759	0.554	1.250	0.383	1.104
P-val :$\kappa = 0$	0.927	0.758	0.279	0.930	0.338

Note: The table presents F statistics and P-values corresponding to the null hypothesis that $\kappa = 0$ (see equation A.12) for several specifications. Column (1) presents results from a model including first lags of capital, labor and intermediate inputs. Columns (2) and (3) present results from specifications that include second and third lags of the variables respectively. Columns(4) and (5) report results from tests which include flexible polynomials of the first lags for capital, labor and intermediate inputs. Robust standard errors are presented in parenthesis.

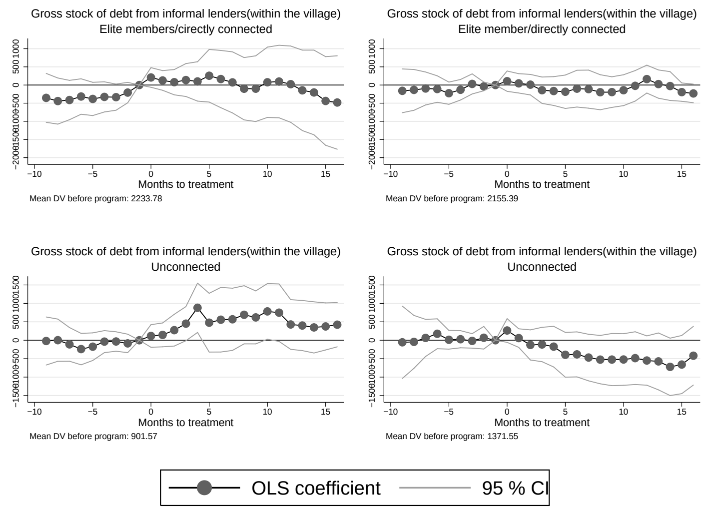
A.3 Robustness to the agricultural cycle and placebo analysis

This section replicates the flexible difference-in-difference results of the paper in the placebo sample following the approach discussed in section 1.7.3. To do so, I use the two years preceding the implementation of the program. In particular I focus on a time window that excludes the data I used to compute my main estimates: $\tau_{v,t} \in [-36,-6)$. I normalize the time-to-treatment variable τ to be between -12 and 17 (centered at -1) such that the calendar months in which the funds were actually released coincide to those in the placebo exercise. For example, if the funds for a certain village were released in June ($\tau_{vt} = 0$), for that same village June would be the first month of treatment in the placebo periods $\tau_{vt}^{PLACEBO} = 0$. The placebo sample coincides with the period September 1999-February 2001.

It is worth mentioning that more placebo months are available for the villages that enter into treatment later, conversely villages that enter the treatment earlier are not observed for all the periods preceding the placebo treatment (i.e. months for which $\tau_{vt} < 0$ in the placebo sample). Appendix Figures A.7 A.8 reproduces the main figure in the paper. They plot the results from the study sample on the left-hand side, and present the placebo results on the right-hand-side. There is a pattern of pre-trends in the placebo sample which could be related to decreases in overall financial activity due to the South-East Asian financial crisis and the associate recovery, or measurement error in the first rounds of the survey. However, the flexible difference-in-difference estimates in the placebo sample look flat in most cases, and, when different from zero, move in the opposite direction of the effects reported in the original analysis suggesting that, if anything, the main estimates understate the true effects.

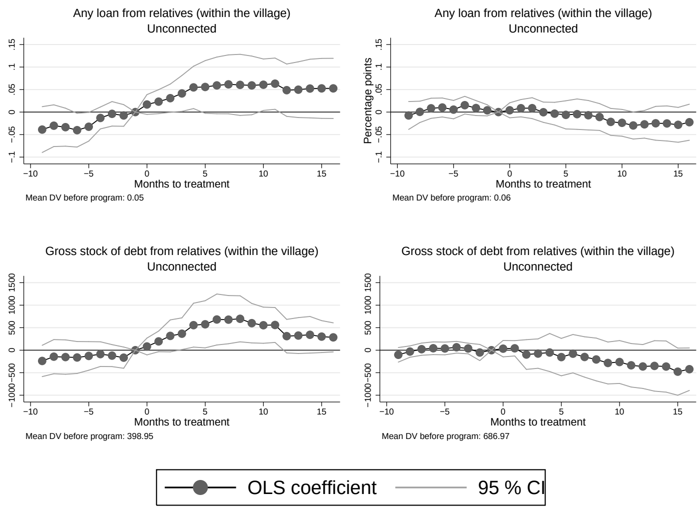
Placebo test for main results

Figure A.7. Short-term on Credit from Local Informal Sources



Note: The figure depicts flexible difference-in-difference estimates corresponding to equation (1.6). The top panel plots OLS coefficients capturing the effects of the program on borrowing from informal lenders (either personal lenders or relatives) by connected households, while the bottom panel presents estimates for total borrowing from informal lenders by unconnected households. The left-hand-side graphs present results related to the implementation of the program, while the graphs in the right-hand panel represent estimates using the placebo sample.

Figure A.8. Short-term Effects on Credit from Relatives- Unconnected Households



Note: The figure depicts flexible difference-in-difference estimates corresponding to equation (1.6). The top panel plots OLS coefficients capturing the effects of the program on borrowing from relatives (number of outstanding loans) by unconnected households, while the bottom panel presents estimates for total borrowing from relatives by unconnected households. The left-hand-side graphs present results related to the implementation of the program, while the graphs in the right-hand panel represent estimates using the placebo sample.

A.3.1 Attrition

Table A.16. Effects on Total Borrowing (Excluding Attriters)

	Panel A: Effects on credit from the program					
	Any credit from MBVF			**Gross debt from MBVF**		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	Connected	Unconnected	All	Connected	Unconnected
$Post_{vt}$	0.363***	0.423***	0.251***	6,320.631***	7,895.595***	2,983.796***
	(0.022)	(0.029)	(0.035)	(446.287)	(604.506)	(531.589)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.012]
Observations	18,305	12,230	6,075	18,232	12,179	6,053
R-squared	0.617	0.635	0.569	0.591	0.616	0.527
Baseline DV mean	0.0301	0.0450	0	45.57	68.19	0
Clusters (# households)	509	340	169	509	340	169

	Panel B: Effects on total credit					
	Any credit			**Total Gross outstanding debt**		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	Connected	Unconnected	All	Connected	Unconnected
$Post_{vt}$	0.073***	0.058***	0.104***	3,435.158	3,899.126	1,858.497
	(0.014)	(0.015)	(0.027)	(2,454.030)	(3,226.529)	(3,709.507)
	[0.000]	[0.000]	[0.012]	[0.232]	[0.356]	[0.612]
Observations	18,305	12,230	6,075	18,205	12,195	6,010
R-squared	0.653	0.623	0.658	0.876	0.833	0.919
Baseline DV mean	0.673	0.745	0.529	65935	61728	74462
Clusters (# households)	509	340	169	509	340	169

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on total borrowing, by connectedness with the local elite. The sample includes only households who are always interviewed during the 172 survey waves. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (2.2)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Panel A reports results for the effect of the rollout of the program on the program's uptake and Panel B shows results for total borrowing (winsorizing the top 1% of observations). Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

Table A.17. Effects on Informal Credit (Excluding Attriters)

Panel A: Any loan from informal lenders

	Connected			Unconnected		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Any informal	Relatives	Non-relatives	Any informal	Relatives	Non-relatives
$Post_{vt}$	-0.007	-0.000	-0.004	0.040**	0.032*	0.014
	(0.014)	(0.011)	(0.012)	(0.020)	(0.016)	(0.012)
	[0.684]	[0.916]	[0.752]	[0.188]	[0.028]	[0.800]
Observations	12,230	12,230	12,230	6,075	6,075	6,075
R-squared	0.703	0.674	0.642	0.600	0.561	0.605
Baseline DV mean	0.164	0.0650	0.111	0.0946	0.0511	0.0488
Clusters (# households)	340	340	340	169	169	169

Panel B: Gross stock of debt with informal lenders

	Connected			Unconnected		
	(1)	(2)	(3)	(4)	(5)	(6)
	Any informal	Relatives	Non-relatives	Any informal	Relatives	Non-relatives
$Post_{vt}$	624.130***	114.186	280.446*	446.749*	409.031**	176.284
	(232.407)	(79.465)	(150.523)	(231.349)	(195.825)	(127.309)
	[0.016]	[0.116]	[0.180]	[0.224]	[0.020]	[0.632]
Observations	12,115	12,168	12,096	6,004	5,995	6,075
R-squared	0.771	0.745	0.711	0.585	0.603	0.598
Baseline DV mean	1791	615.6	963.2	982.3	439	535.3
Clusters (# households)	340	340	339	169	169	169

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on borrowing from informal lenders, by connectedness with the local elites. The sample includes only households who are always interviewed during the 172 survey waves. Informal lenders include personal money lenders and relatives in the village. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (2.2)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Panel A reports results for the number of outstanding loans and Panel B shows results for the gross stock of debt (winsorizing the top 1% of observations). Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

Table A.18. Effects on Total Borrowing by Connectedness Score

	Panel A: Effects on credit from the program					
	Any credit from MBVF			Gross debt from MBVF		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	High	Low	All	High	Low
$Post_{vt}$	0.328***	0.389***	0.267***	3,264.857*	7,929.495***	2,851.297***
	(0.019)	(0.030)	(0.026)	(1,965.708)	(619.981)	(378.041)
	[0.000]	[0.000]	[0.000]	[0.120]	[0.000]	[0.020]
Observations	23,228	11,468	11,760	23,128	11,417	11,738
R-squared	0.613	0.641	0.570	0.866	0.636	0.527
Baseline DV mean	0.0290	0.0574	0.00151	60747	86.47	0.757
Clusters (# households)	671	331	340	671	331	340

	Panel B: Effects on total credit					
	Any credit			Total Gross outstanding debt		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	High	Low	All	High	Low
$Post_{vt}$	0.074***	0.057***	0.098***	5,529.391***	6,392.798**	987.431
	(0.013)	(0.016)	(0.020)	(373.757)	(2,685.509)	(2,734.166)
	[0.000]	[0.004]	[0.008]	[0.000]	[0.088]	[0.752]
Observations	23,228	11,468	11,760	23,155	11,433	11,695
R-squared	0.661	0.595	0.649	0.590	0.786	0.908
Baseline DV mean	0.665	0.821	0.515	42.97	60121	61356
Clusters (# households)	671	331	340	671	331	340

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on total borrowing, by connectedness with the local elite. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (2.2)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Panel A reports results for the effect of the rollout of the program on the program's uptake and Panel B shows results for total borrowing (winsorizing the top 1% of observations). Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). The connectedness score corresponds to an index based on the factor loadings of the first principal component related to all the different types of socioeconomic interactions with local elites. High score: households whose score is above the median. Low: households whose score is below the median.

Table A.19. Effects on Informal Credit by Connectedness Score

Panel A: Any loan from informal lenders

	High connectedness			**Low connectedness**		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Any informal	Relatives	Non-relatives	Any informal	Relatives	Non-relatives
$Post_{vt}$	-0.003	-0.003	-0.007	0.025*	0.025**	0.011
	(0.019)	(0.014)	(0.016)	(0.014)	(0.012)	(0.010)
	[0.932]	[0.876]	[0.692]	[0.404]	[0.076]	[0.816]
Observations	11,468	11,468	11,468	11,760	11,760	11,760
R-squared	0.683	0.646	0.627	0.671	0.630	0.678
Baseline DV mean	0.210	0.0864	0.143	0.116	0.0555	0.0673
Clusters	331	331	331	340	340	340

Panel B: Gross stock of debt with informal lenders

	High connectedness			**Low connectedness**		
	(1)	(2)	(3)	(4)	(5)	(6)
	Any informal	Relatives	Non-relatives	Any informal	Relatives	Non-relatives
$Post_{vt}$	637.667**	291.817*	116.849	404.136**	220.620*	160.543*
	(266.360)	(150.283)	(166.179)	(177.592)	(128.296)	(88.772)
	[0.028]	[0.056]	[0.544]	[0.220]	[0.024]	[0.460]
Observations	11,311	11,330	11,332	11,656	11,664	11,687
R-squared	0.762	0.672	0.695	0.732	0.644	0.644
Baseline DV mean	2272	686.2	1361	1271	464.9	533.1
Clusters	331	331	330	340	340	339

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on borrowing from informal lenders, by connectedness with the local elites. Informal lenders include personal money lenders and relatives in the village. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (1.7)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Panel A reports results for the number of outstanding loans and Panel B shows results for the gross stock of debt (winsorizing the top 1% of observations). Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). he connectedness score corresponds to an index based on the factor loadings of the first principal component related to all the different types of socioeconomic interactions with local elites. High score: households whose score is above the median. Low: households whose score is below the median.

A.4 Appendix: Proofs of propositions 1 and 2

Consider the case of a rural household which chooses the optimal amount of inputs to be used for the family business or farm at the beginning of the year ($t = 0$) and uses the profits and other government transfers to finance consumption in the rest of the year ($t = 1$). These households may finance the only input in this economy (k_{0i}) using their initial exogenous wealth (w_i) or borrow (d_{0i}) at an interest rate of r. However, they may be liquidity constrained and only be able to borrow up to $\bar{d}$, which is exogenously determined and can be expanded by receiving loans from by the MBVF committee (b_i). Households maximize the following simplified problem:

$$\max_{c_{1i},k_{0i},d_{0i}} U(c_{1i}) \tag{A.13}$$

$$\text{s.t.}$$

$$c_{1i} + (1+r)q_i d_{0i} = A_i f(k_{0i}) \tag{A.14}$$

$$p_k k_{0i} \leq w_i + d_{0i} \tag{A.15}$$

$$d_{0i} \leq \bar{d} + b_i \tag{A.16}$$

where U denotes an increasing and concave utility function of consumption in period $t = 1$ (c_{1i}), A_i denotes household total factor productivity associated to the production function $f(k_{0i})$ which is increasing and concave in k.

Assume u is a function of consumption in period t such that $u' > 0$ and $u'' < 0$. f is a production function that transforms the only input (k) into units of consumption goods and is increasing in k and concave ($f'' < 0$). Let $\lambda_1, \lambda_2, \lambda_3$ be the lagrange multipliers associated to constraints (A.14)-(A.16), respectively. The lagrangian function associated to the optimization problem solved by household i is:

$$\mathbf{L} = u(c_{1i}) + \lambda_1(A_i f(k_{0i}) - c_{1i} - (1+r)q_i d_{0i}) + \lambda_2(w_i + d_{0i} - p_k k_{0i}) + \lambda_3(\bar{d} + b_i - d_{0i})$$

The first order conditions imply:

$$u'(c_{1i}) = \lambda_1 \tag{A.17}$$

$$\frac{u'(c_{1i})}{p_k}(A_i f'(k_{0i})) = \lambda_2 \tag{A.18}$$

$$u'(c_{1i})\frac{1}{p_k}(A_i f'(k_{0i}) - p_k(1+r)) = \lambda_3 \tag{A.19}$$

Proof of Proposition 1

Proposition 1 *If households face borrowing constraints, the marginal utility of relaxing this constraint is decreasing in initial wealth. Moreover, the marginal utility of relaxing a household's liquidity constraint is an increasing function of household productivity if the distortion in the optimal choice of inputs is large.*

Proof 1 *In the context of binding liquidity constraints, each households only borrows up to $d^*_{0i} = \bar{d}$ and purchases inputs such that $k^*_{01} = \frac{w_i+\bar{d}+b_i}{p_k}$. Without loss of generality assume $q_i = 1$. Optimal consumption in this case is $c_{1i} = A_i f(\frac{w_i+\bar{d}+b_i}{p_k}) - (1+r)(\bar{d}+b_i)$. As a consequence of the envelop theorem, the marginal utility of loans from the program equals the marginal utility of relaxing the household's liquidity constraint ($\frac{\partial V}{\partial b} = \lambda_3$).*

To see whether λ_3 is an increasing or decreasing function of borrowing from the program b_i, initial wealth w_i, and household productivity A_i, I obtain the respective partial derivatives of λ_3 using equation (A.19).

$$\frac{\partial \lambda_{3i}}{\partial w_i} = \frac{u'' A_i f'}{p_k}(\frac{1}{p_k}(A_i f' - (1+r)p_k)) + u'\frac{A_i f''}{p_k} < 0 \tag{A.20}$$

$$\frac{\partial \lambda_{3i}}{\partial A_i} = \frac{u'' f}{p_k}(A_i f' - p_k(1+r)) + u'\frac{f'}{p_k} \tag{A.21}$$

$$\tag{A.22}$$

Equation (A.20) *is negative because u and f are concave, and because $A_i f' > (1+r)p_k$ when* *liquidity constraints are binding. The intuition is that because households are liquidity constrained, the marginal product of an extra unit of input still exceeds the costs of financing it. The sign of* (A.21) *will depend on the curvature of the utility function and the size of the distortion in the allocation of inputs* $A_i f' - p_k(1+r)$

$$\frac{f'}{f}(A_i f' - p_k(1+r)) > -\frac{u''}{u'} \tag{A.23}$$

Note that this condition will be satisfied depending on the concavity of the utility function. For example, this condition is trivially satisfied if household are simply profit maximizers –i.e., linear utility function–.

Equation (A.20) *implies that the marginal utility from borrowing from the program is decreasing in both borrowing and wealth. Equations* (A.21) *and* (1.2) *imply that households with a higher utility derived from the program are high-productivity households.*

Proof of Proposition 2

Proposition 2 *If households do not face borrowing constraints but face high borrowing interest rates, the marginal utility from a reduction in the interest rate is a decreasing function of initial wealth and an increasing function of household productivity*

Proof 2 *If the liquidity constraints are not binding, then households choose inputs based on prices, interest rates and household productivity (*$k_{0i}^{**} = k(A_i, r, p_k)$*). In this environment, household debt accounts for (*$d_{0i}^{**} = p_k k(A_i, r, p_k) - w_i$*) and the marginal utility of decreasing interest rates is:* $\lambda_1 d_{0i}^{**}$ *and is positive if households are net borrowers (*$d_{0i}^{**} > 0$*). Taking derivatives with respect to* w_i *and* A_i*:*

$$\frac{\partial \lambda_1 d_{0i}^{**}}{\partial w_i} = u''(1+r)d_{0i}^{**} - u' < 0 \tag{A.24}$$

$$\frac{\partial \lambda_1 d_{0i}^{**}}{\partial A_i} = u'' f d_{0i}^{**} + u' p_k \frac{\partial k^{**}}{\partial A_i} \tag{A.25}$$

Equation (A.24) *is negative due to the concavity of u. Equation* (A.25) *is positive if the marginal increase in utility derived from increasing inputs offsets the marginal cost in terms of utility of having to repay debt.*

$$\frac{1}{f} p_k \frac{\partial k^{**}}{\partial A_i} > -u'' d_{0i}^{**}$$

This will typically be true for profit maximizing households–i.e., linear utility function–.

Appendix B

Appendix for Chapter 2

B.1 Supplementary figures and tables

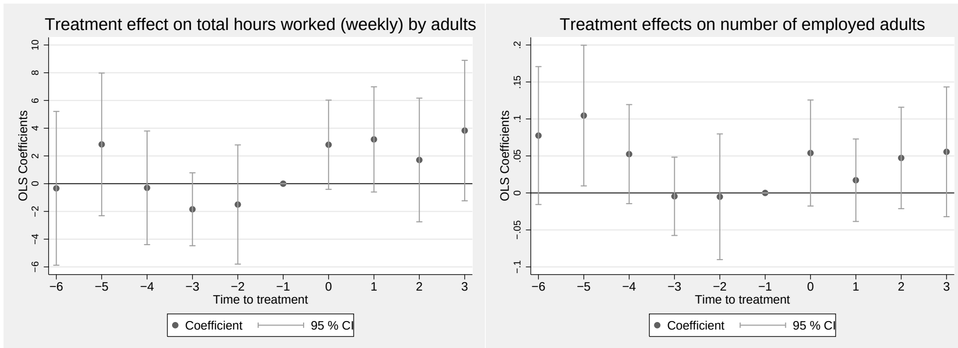
Table B.1. Treatment Effects: Parents of Children from 1st to 8th Grade

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Work Outcomes (Adults - Household)						
	Total hours/week			Total working adults		
TE (DD)	3.322**	2.638**	2.941**	0.045	0.025	0.026
	(1.380)	(1.295)	(1.346)	(0.028)	(0.025)	(0.026)
Observations	30,618	29,502	29,502	30,791	29,663	29,663
R-squared	0.007	0.155	0.156	0.005	0.243	0.243
Mean DV	78.24	78.24	78.24	1.712	1.712	1.712
Panel B: Work Outcomes (Female household heads/ heads' spouses)						
	Hours/week			Worked last week		
TE (DD)	2.418***	2.279***	2.083***	0.033**	0.031**	0.025*
	(0.660)	(0.629)	(0.653)	(0.013)	(0.012)	(0.013)
Observations	29,533	29,518	29,518	29,917	29,902	29,902
R-squared	0.013	0.101	0.102	0.006	0.099	0.099
Mean DV	26.45	26.45	26.45	0.650	0.650	0.650
Panel B: Work Outcomes (Male household heads / heads' spouses)						
	Hours/week			Worked last week		
TE (DD)	1.176	1.450**	1.608**	0.005	0.007	0.009*
	(0.749)	(0.672)	(0.697)	(0.006)	(0.006)	(0.006)
Observations	26,368	25,251	25,251	26,829	25,699	25,699
R-squared	0.007	0.088	0.088	0.003	0.066	0.067
Mean DV	47.50	47.50	47.50	0.948	0.948	0.948
Controls	NO	YES	YES	NO	YES	YES
Municipality FE	NO	YES	YES	NO	YES	YES
Group Trend	NO	NO	YES	NO	NO	YES
Clusters	293	293	293	293	293	293

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

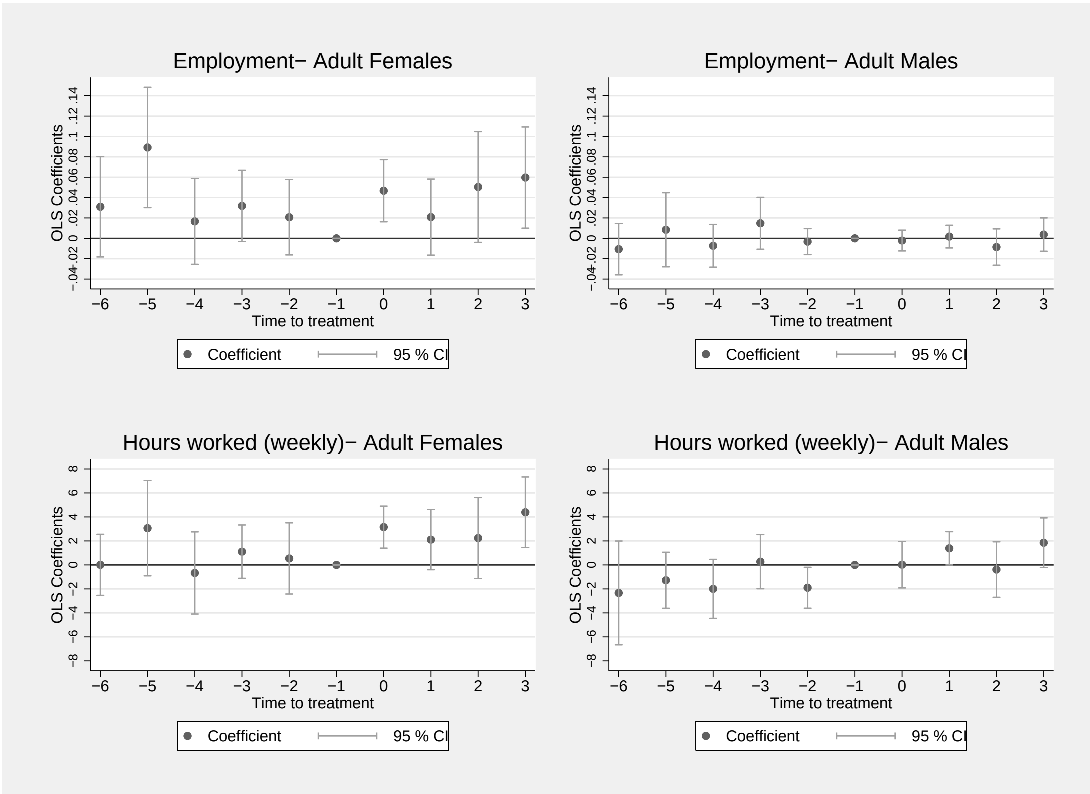
Note: The table presents OLS estimates for a difference-in-difference model. The coefficients represent differential changes in labor supply before and after the program between exposed and non exposed-children. Standard errors, clustered at the municipality level, are presented in parentheses. Panel A presents treatment effects concerning aggregate data at the household level. Panels B and C present treatment effects regarding employment for female heads of household or spouses and male heads of household or spouses, respectively. The sample includes children from 1st to 8th grade.

Figure B.1. Treatment Effects on Total Household Labor Supply (adults): Total Weekly Work Hours (left) and Number of Adults Working



The figure depicts OLS coefficients from equation (2.1). Left-hand panel: each coefficient estimates differences in differences on hours worked by adults between the treatment and control group with respect to the period just before the program was implemented ($\tau = -1$). The dependent variable measures the total number of hours worked by adults in child *i*'s household. Standard errors are clustered at the municipality level. Right-hand panel: Each coefficient estimates differences in differences on adult employment between the treatment and control group with respect to the period just before the program was implemented ($\tau = -1$). The dependent variable measures the number of adults employed in child *i*'s household. The estimation sample includes all potential beneficiary children from 1st grade to 8th grade. Standard errors are clustered at the municipality level.

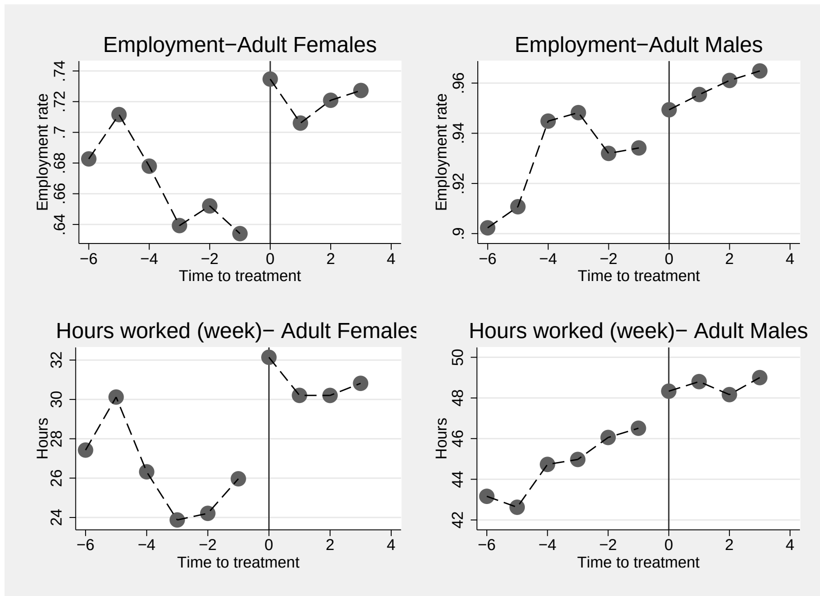
Figure B.2. Effects on Employment for Adults (1st-8th grade)



The figure depicts OLS coefficients from equation (2.1). Each coefficient estimates differences in differences on the relevant measure of labor supply between the treatment and control group with respect to the period just before the program was implemented ($\tau = -1$). The plots on the left present results for adult males, while the plots on the right present results for adult females. Standard errors are clustered at the municipality level. The estimation sample includes all potential beneficiary children from 1st grade to 8th grade.

B.1.1 Effects Excluding Children with Siblings with Different Treatment Status

Figure B.3. Employment and Work Hours (weekly) for Adults



The figure replicates the main event-study analyses focusing in a reduced sample of children whose siblings treatment status is the same as theirs. The top panels depict employment rate for adult males (heads of household or spouses) and adult females (heads of household or spouses) in child *i*'s household before and after child *i* is exposed to treatment. The bottom panel depicts weekly hours for both adult males and females. Time to treatment is equal to 0 in the first period in which treatment kicks in. Children who have siblings with different treatment status are excluded from the sample.

Table B.2. Effects Excluding Children with Siblings with Different Treatment Status

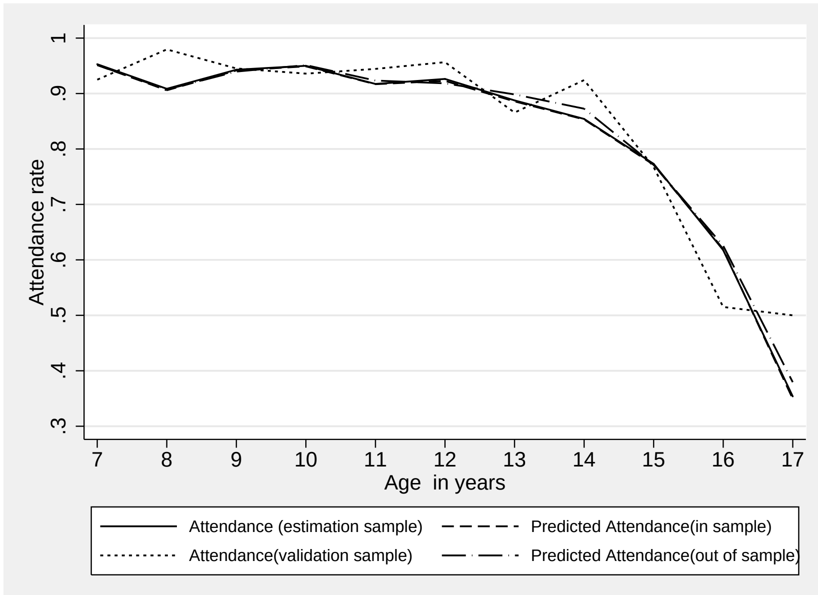
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Work Outcomes (Adults - Household)						
	Total hours/week			Total working adults		
TE (DD)	3.847	1.616	2.387	0.069	0.007	0.014
	(2.825)	(2.597)	(2.912)	(0.056)	(0.057)	(0.065)
Observations	9,112	8,624	8,624	9,178	8,687	8,687
R-squared	0.010	0.192	0.194	0.010	0.287	0.290
Mean DV	75.04	75.04	75.04	1.654	1.654	1.654
Panel B: Work Outcomes (Female household heads/heads' spouses)						
	Hours/week			Worked last week		
TE (DD)	4.853***	4.700***	4.743***	0.069**	0.061*	0.058
	(1.419)	(1.403)	(1.539)	(0.031)	(0.032)	(0.036)
Observations	8,639	8,632	8,632	8,766	8,759	8,759
R-squared	0.016	0.116	0.117	0.007	0.117	0.119
Mean DV	25.97	25.97	25.97	0.634	0.634	0.634
Panel B: Work Outcomes (Male household heads/heads' spouses)						
	Hours/week			Worked last week		
TE (DD)	-0.298	-0.397	0.459	0.001	-0.005	0.007
	(1.498)	(1.587)	(1.566)	(0.019)	(0.016)	(0.017)
Observations	7,562	7,075	7,075	7,685	7,193	7,193
R-squared	0.010	0.110	0.111	0.008	0.104	0.106
Mean DV	46.51	46.51	46.51	0.934	0.934	0.934
Controls	NO	YES	YES	NO	YES	YES
Municipality FE	NO	YES	YES	NO	YES	YES
Group Trend	NO	NO	YES	NO	NO	YES
Clusters	286	286	286	286	286	286

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: The table presents OLS estimates for a difference-in-difference model under alternative specifications. The table replicates the main results excluding those children who have siblings in a different treatment group. The coefficients represent differential changes in labor supply before and after the program between exposed and non-exposed children. Standard errors, clustered at the municipality level, are presented in parentheses. Panel A presents treatment effects concerning aggregate data at the household level. Panels B and C present treatment effects regarding employment for female heads of household or spouses and male heads of households or spouses, respectively.

B.1.2 Heterogeneous Treatment Effects by Counterfactual Attendance Rate

Figure B.4. Predicted and Observed Attendance Rates



This picture depicts attendance rates as a function of age for the 2004 wave for the estimation sample and the validation sample. Attendance rate is depicted for actual and predicted data. The probit model included age fixed effects, years of schooling fixed effects, and demographic characteristics. 80% of the 2004 observations were randomly assigned to an estimation sample, the remaining were assigned to a validation sample. The table shows that the model performs well when it comes to out-of-sample prediction.

Table B.3. Heterogeneous Treatment Effects by Counterfactual Attendance Rate

	(1)	(2)	(3)	(4)
	Total adults		**Males hh heads**	
VARIABLES	**Hours/week**	**Worked**	**Hours/week**	**Worked**
TE (DD)	2.679	0.033	0.737	-0.005
	(1.834)	(0.034)	(0.848)	(0.007)
TE x 1[Attendance rate¡0.8] (DDD)	-5.878	-0.144*	1.812	0.015
	(4.800)	(0.077)	(2.064)	(0.018)
Observations	17,434	17,543	14,747	15,010
R-squared	0.164	0.254	0.093	0.075
Clusters	290	290	289	289
Mean DV	79.37	1.732	47.86	0.949
Mean Covariate	0.154	0.154	0.154	0.154
TE at CV=1	-3.199	-0.111*	2.549	0.0103
p-val	0.423	0.0878	0.183	0.546

*** p¡0.01, ** p¡0.05, * p¡0.1

Note: The table presents OLS estimates for a triple-difference model. Standard errors, clustered at the municipality level, are presented in parentheses. The coefficients in the first row represent treatment effects when the relevant covariate equals 0 (DD) (inframarginal children). The estimates in the second row report heterogeneity by counterfactual predicted attendance rate based on a probit model estimated for the 2004 sample. Treatment effects for marginal children are presented in the bottom panel.

Table B.4. Treatment Effects on Enrollment and Employment (Children)

Panel A: Enrollment and child employment

	Enrollment			Worked last week		
TE (DD)	0.037**	0.040***	0.017	0.010	0.004	0.007
	(0.017)	(0.014)	(0.013)	(0.018)	(0.015)	(0.016)
Observations	15,164	14,519	14,519	18,447	17,678	17,678
R-squared	0.015	0.321	0.322	0.011	0.315	0.315
Mean DV (Baseline)	0.900	0.900	0.900	0.294	0.294	0.294
Controls	NO	YES	YES	NO	YES	YES
Municipality FE	NO	YES	YES	NO	YES	YES
Group Trend	NO	NO	YES	NO	NO	YES
Clusters	289	289	289	290	290	290

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: The table presents difference-in-difference estimates (DD) for the probability of enrollment and the probability that child *i* reported working the week preceding the interview. Note that since the surveys report enrollment at the beginning of each school year, treatment effects on enrollment are identified using eligibility in the year preceding the survey. For example, a child who has completed 5th grade in 2005 and is observed in the 2006 wave will be in the control group for that year. Conversely, a child who completed 4th in 2005 and is observed in the 2006 sample will be in the treatment group.

Table B.5. Adult females: Heterogeneous Treatment Effects by Access to Credit

	(1)	(2)	(3)	(4)
	Total adults		**Males hh heads**	
	Hours/week	**Worked**	**Hours/week**	**Worked**
TE (DD)	2.850	0.013	3.301	0.023
	(2.219)	(0.015)	(4.602)	(0.080)
TE x # branches per 100000 people (DDD)	0.140	-0.001	-0.101	-0.007
	(0.092)	(0.001)	(0.229)	(0.005)
Observations	10,738	10,967	12,811	12,895
R-squared	0.088	0.077	0.144	0.233
Controls	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES
Group Trend	NO	NO	NO	NO
Area-cohort-year FE	YES	YES	YES	YES
Clusters	98	98	98	98
Mean DV	0.662	0.662	0.662	0.662
Mean # branches per 100000 people	9.341	9.341	9.341	9.341

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: The table presents difference-in-difference estimates (DD) and triple-difference estimates (DDD) in the first and second row, respectively. The number of financial branches per 100,000 individuals in each municipality is used as a third source of variation. Data regarding financial branches correspond to 2005, the year before the program's implementation, and is only available for the municipalities that are provincial capitals. The sample for these regressions accounts for one-third of the clusters' sample but two-thirds of the total observations.

Table B.6. Effects on Self Employment-Adult Females

	(1)	(2)	(3)	(4)
	Total adults		**Male hh heads**	
	Self-employed	**Works at home**	**Self-employed**	**Works at home**
TE (DD)	0.038*	-0.003	0.008	-0.083
	(0.020)	(0.008)	(0.012)	(0.063)
Observations	17,506	17,554	7,923	1,128
R-squared	0.117	0.071	0.143	0.332
Controls	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES
Group Trend	YES	YES	YES	YES
Clusters	290	290	281	116
Mean DV	0.691	0.0520	0.898	0.309

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: The table presents OLS estimates for a difference-in-difference model. The coefficients represent differential changes in the number of self-employed adults in the household before and after the program for exposed and non-exposed children, and differential changes in the self-employment probability before and after the program between male heads of household from exposed and non-exposed children, respectively. Standard errors, clustered at the municipality level, are presented in parentheses. The dependent variable is denoted as 1 if the head of household is self-employed and 0 if they did not report working the week preceding the survey.

Appendix C

Appendix for Chapter 3

C.1 Supplementary figures and tables

Table C.1. Correlation between beliefs and value-added

	(1)	(2)	(3)	(4)
VARIABLES	log Value Added			
Log beliefs	0.0530***	0.0296**	0.0512***	0.0334**
	(0.0170)	(0.0143)	(0.0167)	(0.0150)
Observations	1,915	1,911	1,915	1,911
R-squared	0.010	0.137	0.155	0.240
Control for capital	No	Yes	No	Yes
Village F.E.	No	No	Yes	Yes

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents estimates of a regression of value-added in logs on household beliefs regarding current profits for several specifications. Standard errors are clustered at the household level to account for serial correlation. Beliefs are measured as the self-reported household projected income for period t predicted in $t-1$.

Table C.2. Correlation between Within-village Productivity Rankings

	(1)	(2)
	Percentile ranking - Proxy-variable Method	
Percentile ranking - Fixed Effects Method	0.470***	0.499***
	(0.0315)	(0.0341)
Constant	0.331***	0.316***
	(0.0187)	(0.0165)
Observations	821	821
R-squared	0.208	0.266
Village FE	NO	YES

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents correlations between within-village productivity rankings obtained by the proxy-variable method and the fixed effects method. Standard errors are clustered at the village level.

Table C.3. Correlates of baseline productivity and demographic characteristics

	Proxy-variable			Fixed-effects		
	(1)	(2)	(3)	(4)	(5)	(6)
	Productivity	Productivity Rank	High-productivity	Productivity	Productivity Rank	High-productivity
Number of Adult Males in Household	0.0626**	0.0199*	0.0159	0.158***	0.0335***	0.0516***
	(0.0305)	(0.0104)	(0.0185)	(0.0417)	(0.0111)	(0.0179)
Number of Adult Females in Household	0.259***	0.0923***	0.126***	0.271***	0.0432***	0.0673***
	(0.0333)	(0.0118)	(0.0235)	(0.0505)	(0.0129)	(0.0219)
Number of Children in Household	-0.0638***	-0.00754	0.00103	0.0295	0.00666	-0.00554
	(0.0239)	(0.00795)	(0.0136)	(0.0285)	(0.00743)	(0.0129)
Dummy: Male Head of Household	0.146*	0.0438	0.0515	0.188*	0.0448	0.0394
	(0.0742)	(0.0270)	(0.0426)	(0.106)	(0.0280)	(0.0425)
Head's main occupation: Farm (agriculture/livestock)	0.254***	0.0447*	0.0965**	0.371***	0.0572**	0.136***
	(0.0732)	(0.0243)	(0.0408)	(0.123)	(0.0260)	(0.0471)
Number of Businessowners in Household	0.340***	0.0591***	0.0588*	0.370***	0.0618***	0.0718**
	(0.0637)	(0.0215)	(0.0334)	(0.100)	(0.0197)	(0.0322)
Age of Head of Household	-0.00481*	0.0000670	0.00229*	-0.0166***	-0.00253***	-0.00334**
	(0.00245)	(0.000820)	(0.00136)	(0.00320)	(0.000716)	(0.00136)
Years of schooling - HH head	0.0253**	0.0122***	0.0163**	0.0357**	0.00649*	0.00961*
	(0.00975)	(0.00338)	(0.00622)	(0.0149)	(0.00327)	(0.00516)
Observations	886	886	886	811	811	811
R-Squared	0.257	0.134	0.0845	0.218	0.127	0.0893

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table reports within village correlations between estimates of productivity and demographic characteristics. All regressions include village fixed-effects. Standard errors are clustered at the village level.

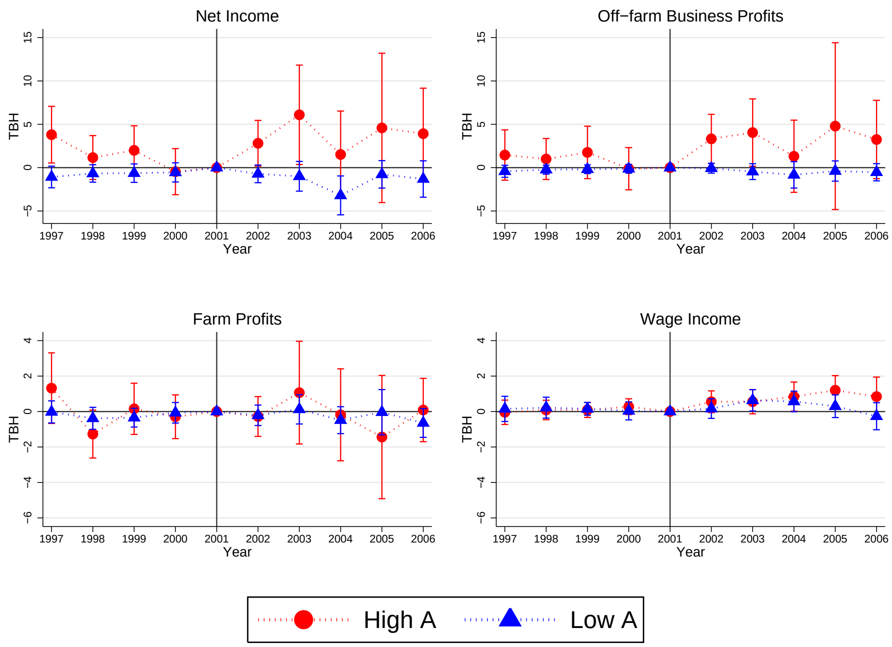


Figure C.1. Effects of program rollout on household income - Fixed-effects approach

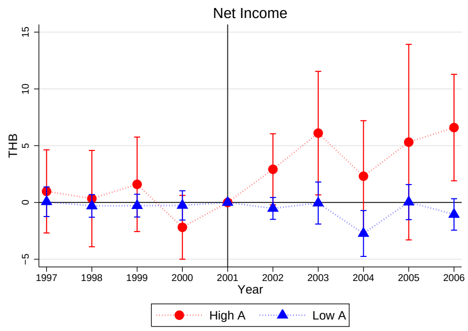
Note: The figure depicts flexible estimates corresponding to the specification in (3.5).Each dot represents differences in income between households from villages with high and low per-capita program funds, for each year with respect to the year preceding the announcement of the program (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per-capita TBH in each village on the corresponding outcome. High A: household belongs to the top-third tercile of the baseline productivity distribution in each village. Low A: households belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control function approach using household beliefs about profits as a proxy variable. 95 % confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design.

Table C.4. Reduced-form effects of the program on off-farm business activities- Winsorized

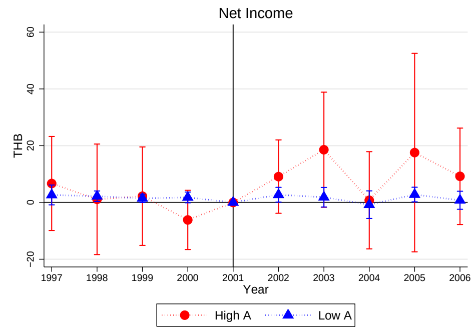
	(1) VF short-term credit	(2) Total short-term credit	(3) Profits	(4) Non-wage Expenses	(5) Wage Expenses	(6) Assets
Panel A: Proxy-variable approach						
Post X Inv HH X High Productivity	-0.0476	1.102*	5.680***	1.406	0.294**	9.241***
	(0.150)	(0.570)	(1.559)	(4.416)	(0.125)	(2.751)
	[0.150]	[0.718]	[2.022]	[4.145]	[0.155]	[2.672]
Post X Inv HH	0.566***	0.624	-1.009	-1.570	-0.0204	-0.964
	(0.187)	(0.426)	(0.766)	(1.245)	(0.0443)	(0.794)
	[0.213]	[0.580]	[0.699]	[1.730]	[0.079]	[1.854]
Effect-High Productivity	0.518***	1.726**	4.671**	-0.165	0.273**	8.277***
SE	0.156	0.592	1.446	4.303	0.123	3.137
SE (bootstrap)	[0.185]	[0.590]	[1.898]	[4.046]	[0.128]	[2.611]
Baseline mean (DV)	11.75	19149.5	31903.0	92164.4	3362.5	92236.6
Observations	2190	2190	2190	2190	2190	2190
Number of households	229	229	229	229	229	229
R-Squared	0.595	0.522	0.391	0.548	0.525	0.645
Panel B: Fixed-effects approach						
	(1) VF short-term credit	(2) Total short-term credit	(3) Profits	(4) Non-wage Expenses	(5) Wage Expenses	(6) Assets
Post X Inv HH X High Productivity	-0.407***	0.328	4.042**	0.396	0.350**	6.046**
	(0.141)	(0.632)	(1.602)	(4.983)	(0.135)	(2.561)
Post X Inv HH	0.685***	0.930***	-0.294	-1.224	-0.0328	0.454
	(0.152)	(0.287)	(0.760)	(0.904)	(0.0442)	(1.698)
Effect High Productivity	0.278	1.257	3.748	-0.829	0.318	6.500
SE	0.229	0.801	1.567	4.847	0.132	2.491
Baseline mean (DV)	11.75	19149.5	31903.0	92164.4	3362.5	92236.6
Observations	2190	2190	2190	2190	2190	2190
Number of households	229	229	229	229	229	229
R-Squared	0.597	0.523	0.386	0.547	0.523	0.640

$***p < 0.01, **p < 0.05, *p < 0.1$

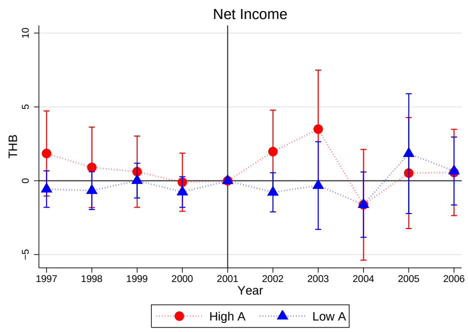
Note: The table reports the reduced-form estimates of the effects of the program as a function of productivity estimated through the proxy-variable approach (Panel A) and the fixed-effects approach (Panel B). The coefficients have been scaled down by 1,000,000 in order to capture the effect of an additional per-capita TBH in each village on the corresponding outcome. The dependent variables are winsorized with respect to the 1st and 99th percentile, respectively. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Panel A also presents bootstrap standard errors in brackets, which are computed using 500 bootstrap samples and clustered at the village level. The estimating sample includes only household who reported owning business assets the year preceding the rollout of the program.



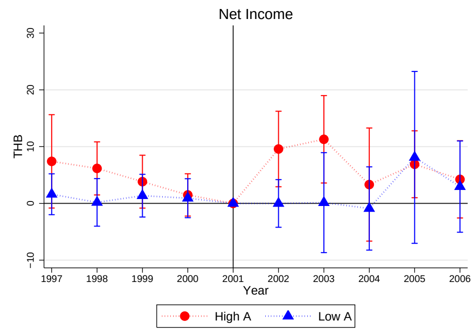
(a) Full sample - Including number of workers



(b) Pre-existing Businesses - Including number of workers



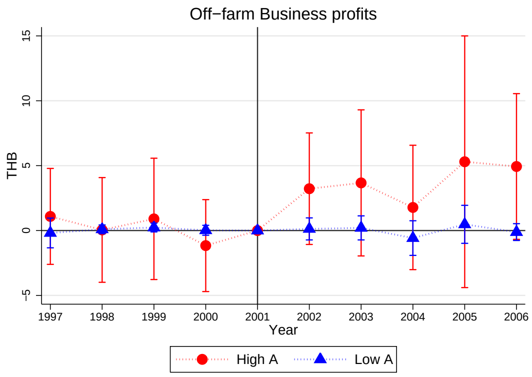
(c) Full sample - Per-capita Model



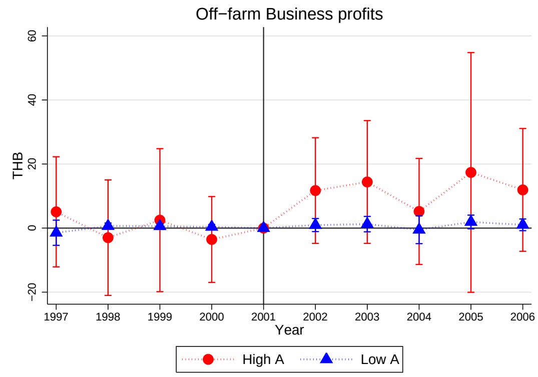
(d) Pre-existing Businesses - Per-capita Model

Figure C.2. Effects of program rollout on household income

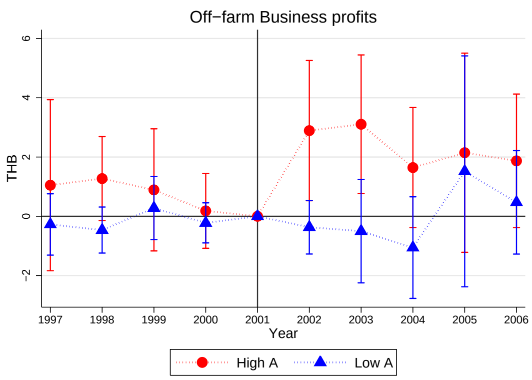
Note: The figure depicts flexible estimates corresponding to the specification in (3.5). Each dot represents differences in income between households from villages with high and low per-capita program funds, for each year with respect to the year of the of announcement of the program (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per-capita TBH in each village on the corresponding outcome. High A: household belongs to the top-third tercile of the baseline productivity distribution in each village. Low A: households belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control function approach using household beliefs about profits as a proxy variable. 95 % confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design.



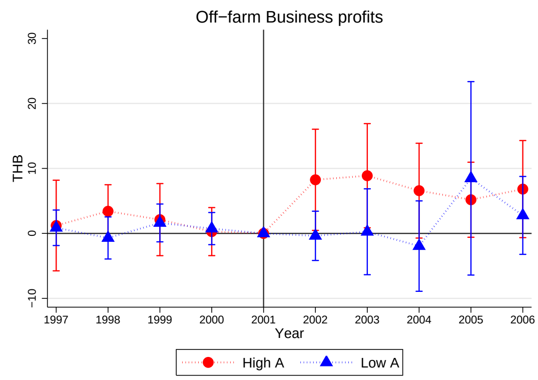
(a) Full sample - Including number of workers



(b) Pre-existing Businesses - Including number of workers



(c) Full sample - Per-capita Model



(d) Pre-existing Businesses - Per-capita Model

Figure C.3. Effects of program rollout on business profits

Note: The figure depicts flexible estimates corresponding to the specification in (3.5). Each dot represents differences in income between households from villages with high and low per-capita program funds, for each year with respect to the year in which the program was announced (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per-capita TBH in each village on the corresponding outcome. High A: household belongs to the top-third tercile of the baseline productivity distribution in each village. Low A: households belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control function approach using household beliefs about profits as a proxy variable. 95 % confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design.

Table C.5. IV Estimates of the Effects of Program Credit on Income and Profits

Panel A: Effects on Income (full sample)

	(1)	(2)	(3)	(4)	(5)	(6)
	Household Income		Wage Income		Total Profits	
	Raw	Winsorized	Raw	Winsorized	Raw	Winsorized
VF Short Term Credit * High Productivity	3.321***	2.445*	0.663	0.585	3.879**	1.768***
	(2.031)	(1.282)	(0.606)	(0.567)	(1.597)	(0.653)
	[1.930]	1.1321	[0.717]	0.6965	[1.871]	0.9992
VF Short Term Credit	-1.355	-1.376**	0.216	0.188	-1.072	-1.086***
	(0.833)	(0.627)	(0.338)	(0.329)	(0.692)	(0.368)
	[0.918]	0.5747	[0.408]	0.4034	[0.864]	0.398
Effect- High Productivity	1.966	1.069	0.878*	0.773*	2.806	0.681
SE	1.631	0.963	0.418	0.358	1.513	0.586
SE bootstrap	[1.709]	[0.991]	[0.514]	[0.469]	[1.739]	0.9009

Panel B: Effects on Profits by source (full sample)

	(1)	(2)	(3)	(4)	(5)	(6)
	Farm		Fishing/Shrimping		Off-farm Business	
	Raw	Winsorized	Raw	Winsorized	Raw	Winsorized
VF Short Term Credit * High Productivity	0.524	0.377	-0.141	0.0151	3.496***	1.376***
	(0.526)	(0.442)	(0.168)	(0.0208)	(1.456)	(0.526)
	[0.642]	0.4781	[0.235]	0.06	[1.691]	0.8501
VF Short Term Credit	-0.350	-0.569*	-0.0443	-0.0265	-0.678	-0.491
	(0.461)	(0.321)	(0.112)	(0.0445)	(0.655)	(0.397)
	[0.488]	0.3935	[0.108]	0.0572	[0.812]	0.2868
Effect- High Productivity	0.173	-0.192	-0.185	-0.0115	2.818*	0.885
SE	0.519	0.367	0.243	0.0320	1.243	0.445
SE bootstrap	[0.662]	[0.399]	[0.317]	[0.036]	[1.463]	[0.785]
First-stage F-stat: Short Term Credit	30.75					
First-Stage F-stat: Interaction	8.030					
Observations	8650					
Number of households	914					

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table reports the instrumental-variables estimates of the effects of program credit as a function of productivity estimated through the proxy-variable approach Panel A presents effects on income by source and Panel B presents effects on profits by type of activity. Odd-numbered columns present results after winsorizing the dependent variable with respect to the 1st and 99th percentile. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrap standard errors are presented in brackets and are computed using 500 bootstrap samples and clustered at the village level. Short-term credit involves program loans with a term shorter than a year. Household profits include farm, fishing and shrimping and off-farm business profits.

Table C.6. IV Estimates of the Effect of Program Credit on Off-farm Businesses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Profits		Non-wage Expenses		Wage Expenses		Assets	
	Raw	Winsorized	Raw	Winsorized	Raw	Winsorized	Raw	Winsorized
VF Short Term Credit * High Productivity	15.52***	7.328***	3.110	2.692	0.838*	0.410**	24.31**	13.49***
	(6.474)	(1.793)	(8.165)	(6.395)	(0.453)	(0.158)	(14.46)	(5.386)
	[5.826]	[2.192]	[7.377]	[5.159]	[0.442]	[0.197]	[10.838]	[3.622]
VF Short Term Credit	-1.349	-1.588	-6.187*	-1.977	-0.160	-0.0475	-0.522	-1.321
	(2.050)	(1.092)	(3.735)	(2.138)	(0.177)	(0.0685)	(1.265)	(1.231)
	[3.535]	[1.332]	[4.191]	[2.100]	[0.367]	[0.126]	[3.596]	[2.462]
Effect- High Productivity	14.17***	5.740**	-3.077	0.715	0.677*	0.363**	23.78**	12.17***
SE	6.177	1.914	7.552	5.961	0.481	0.171	14.84	5.809
SE bootstrap	[4.462]	[2.064]	[6.726]	[4.623]	[0.396]	[0.165]	[10.158]	[3.196]
First-stage F-stat: Short-term credit	13.11							
First-Stage F-stat: Interaction	8.146							
Observations	2188							
Number of households	228							

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table reports instrumental-variables estimates of the effects of program credit as a function of productivity estimated through the proxy-variable approach. Odd-numbered columns present results after winsorizing the dependent variable with respect to the 1st and 99th percentile. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrap standard errors are presented in brackets and are computed using 500 bootstrap samples and clustered at the village level. Short-term credit involves program loans with a term shorter than a year. The estimating sample includes household with pre-existing businesses only. The dependent variables have been winsorized with respect to the 1st and 99th percentile.

Bibliography

Acemoglu, D. (2010). Theory, general equilibrium, and political economy in development economics. *The Journal of Economic Perspectives 24*(3), 17–32.

Ackerberg, D. A., K. Caves, and G. Frazer (2015). Identification properties of recent production function estimators. *Econometrica 83*(6), 2411–2451.

Alatas, V., A. Banerjee, A. G. Chandrasekhar, R. Hanna, and B. A. Olken (2012, August). Network structure and the aggregation of information: Theory and evidence from indonesia. Working Paper 18351, National Bureau of Economic Research.

Alatas, V., A. Banerjee, R. Hanna, B. A. Olken, and J. Tobias (2012, June). Targeting the poor: Evidence from a field experiment in indonesia. *American Economic Review 102*(4), 1206–40.

Alderman, H. and C. H. Paxson (1994). *Do the Poor Insure? A Synthesis of the Literature on Risk and Consumption in Developing Countries*, pp. 48–78. London: Palgrave Macmillan UK.

Alzua, M. L., G. Cruces, and L. Ripani (2013). Welfare programs and labor supply in developing countries: experimental evidence from latin america. *Journal of Population Economics 26*(4), 1255–1284.

Anderson, S., P. Francois, and A. Kotwal (2015, June). Clientelism in indian villages. *American Economic Review 105*(6), 1780–1816.

Anderson, T. and C. Hsiao (1982). Formulation and estimation of dynamic models using panel data. *Journal of Econometrics 18*(1), 47 – 82.

Angelucci, M. and G. De Giorgi (2009). Indirect effects of an aid program: How do cash transfers affect ineligibles' consumption? *American Economic Review 99*(1), 486–508.

Angelucci, M., D. Karlan, and J. Zinman (2015, January). Microcredit impacts: Evidence from a randomized microcredit program placement experiment by compartamos banco. *American Economic Journal: Applied Economics 7*(1), 151–82.

Armendáriz de Aghion, B. and J. Murdoch (2004). *Microfinance: Where do we Stand?*, pp.

135–148. London: Palgrave Macmillan UK.

Asher, S. and P. Novosad (2017, January). Politics and local economic growth: Evidence from india. *American Economic Journal: Applied Economics 9*(1), 229–73.

Attanasio, O., B. Augsburg, R. De Haas, E. Fitzsimons, and H. Harmgart (2015, January). The impacts of microfinance: Evidence from joint-liability lending in mongolia. *American Economic Journal: Applied Economics 7*(1), 90–122.

Augsburg, B., R. De Haas, H. Harmgart, and C. Meghir (2015, January). The impacts of microcredit: Evidence from bosnia and herzegovina. *American Economic Journal: Applied Economics 7*(1), 183–203.

Baird, S., C. McIntosh, and B. Ozler (2011). Cash or condition? evidence from a cash transfer experiment. *The Quarterly Journal of Economics 126*(4), 1709–1753.

Bandiera, O., R. Burgess, N. Das, S. Gulesci, I. Rasul, and M. Sulaiman (2017). Labor markets and poverty in village economies*. *The Quarterly Journal of Economics 132*(2), 811–870.

Banerjee, A., E. Breza, E. Duflo, and C. Kinnan (2015). Do credit constraints limit entrepreneurship? heterogeneity in the returns to microfinance. *Working Paper*.

Banerjee, A., E. Duflo, R. Glennerster, and C. Kinnan (2015, January). The miracle of microfinance? evidence from a randomized evaluation. *American Economic Journal: Applied Economics 7*(1), 22–53.

Banerjee, A., E. Duflo, N. Goldberg, D. Karlan, R. Osei, W. Parienté, J. Shapiro, B. Thuysbaert, and C. Udry (2015). A multifaceted program causes lasting progress for the very poor: Evidence from six countries. *Science 348*(6236).

Banerjee, A., R. Hanna, G. Kreindler, and B. Olken (2015, December). Debunking the Stereotype of the Lazy Welfare Recipient: Evidence from Cash Transfer Programs Worldwide. Working Paper Series 076, Harvard Kennedy School.

Banerjee, A., D. Karlan, and J. Zinman (2015, January). Six randomized evaluations of microcredit: Introduction and further steps. *American Economic Journal: Applied Economics 7*(1), 1–21.

Banerjee, A. V. and E. Duflo (2010, Summer). Giving Credit Where It Is Due. *Journal of Economic Perspectives 24*(3), 61–80.

Banerjee, A. V. and E. Duflo (2014). Do firms want to borrow more? testing credit constraints using a directed lending program. *The Review of Economic Studies 81*(2), 572–607.

Banerjee, A. V. and S. Mullainathan (2008). Limited attention and income distribution. *American Economic Review 98*(2), 489–93.

Banerjee, A. V. and A. F. Newman (1993, April). Occupational Choice and the Process of Development. *Journal of Political Economy 101*(2), 274–98.

Bardhan, P. and D. Mookherjee (2005). Decentralizing antipoverty program delivery in developing countries. *Journal of Public Economics 89*(4), 675 – 704. Cornell - ISPE Conference on Public Finance and Development.

Bardhan, P. and D. Mookherjee (2006a). Decentralisation and accountability in infrastructure delivery in developing countries*. *The Economic Journal 116*(508), 101–127.

Bardhan, P. and D. Mookherjee (2006b). Pro-poor targeting and accountability of local governments in west bengal. *Journal of Development Economics 79*(2), 303 – 327. Special Issue in honor of Pranab Bardhan.

Basurto, P. M., P. Dupas, and J. Robinson (2017, May). Decentralization and Efficiency of Subsidy Targeting: Evidence from Chiefs in Rural Malawi. NBER Working Papers 23383, National Bureau of Economic Research, Inc.

Beaman, L., D. Karlan, B. Thuysbaert, and C. Udry (2014, August). Self-selection into credit markets: Evidence from agriculture in mali. Working Paper 20387, National Bureau of Economic Research.

Becker, G. S. (1965). A theory of the allocation of time. *The Economic Journal 75*(299), pp. 493–517.

Benhassine, N., F. Devoto, E. Duflo, P. Dupas, and V. Pouliquen (2015). Turning a shove into a nudge? a ”labeled cash transfer” for education. *American Economic Journal: Economic Policy 7*(3), 86–125.

Blattman, C., N. Fiala, and S. Martinez (2014). Generating skilled self-employment in developing countries: Experimental evidence from uganda *. *The Quarterly Journal of Economics 129*(2), 697–752.

Blundell, R. and S. Bond (2000). Gmm estimation with persistent panel data: an application to production functions. *Econometric Reviews 19*(3), 321–340.

Boonperm, J., J. Haughton, and S. R. Khandker (2013). Does the village fund matter in thailand? evaluating the impact on incomes and spending. *Journal of Asian Economics 25*, 3 – 16.

Borusyak, K. and X. Jaravel (Working Paper, 2016). Revisiting event study designs.

Breza, E., R. Townsend, and D. Vera-Cossio (2017). Access to credit and productivity: Evidence from thai villages. Work in progress.

Bryan, G., S. Chowdhury, and A. M. Mobarak (2014). Underinvestment in a profitable technology: The case of seasonal migration in bangladesh. *Econometrica 82*(5), 1671–1748.

Cameron, C. A. and D. L. Miller (2015). A practitioners guide to cluster-robust inference. *Journal of Human Resources 50*(2), 317–372.

Cascio, E. U. (2009). Maternal labor supply and the introduction of kindergartens into american public schools. *Journal of Human Resources 44*(1), 140–170.

Chandrasekhar, A. G. and R. Lewis (2017). Econometrics of sampled networks. Technical report.

Crpon, B., F. Devoto, E. Duflo, and W. Parient (2015, January). Estimating the impact of microcredit on those who take it up: Evidence from a randomized experiment in morocco. *American Economic Journal: Applied Economics 7*(1), 123–50.

Cruz, C., J. Labonne, and P. Querubn (2017, October). Politician family networks and electoral outcomes: Evidence from the philippines. *American Economic Review 107*(10), 3006–37.

Dasgupta, P. and D. Ray (1986). Inequality as a determinant of malnutrition and unemployment: Theory. *The Economic Journal 96*(384), pp. 1011–1034.

de Brauw, A., D. O. Gilligan, J. Hoddinott, and S. Roy (2015). Bolsa famlia and household labor supply. *Economic Development and Cultural Change 63*(3), 423–457.

de Brauw, A. and J. Hoddinott (2011). Must conditional cash transfer programs be conditioned to be effective? the impact of conditioning transfers on school enrollment in mexico. *Journal of Development Economics 96*(2), 359 – 370.

De Loecker, J. (2011). Product differentiation, multiproduct firms, and estimating the impact of trade liberalization on productivity. *Econometrica 79*(5), 1407–1451.

de Mel, S., D. McKenzie, and C. Woodruff (2008, November). Returns to Capital in Microenterprises: Evidence from a Field Experiment. *The Quarterly Journal of Economics 123*(4), 1329–1372.

Deininger, K. (2013). Evaluating program impacts on mature self-help groups in india. *World Bank Economic Review 27*(2), 272–296.

Fafchamps, M., D. McKenzie, S. Quinn, and C. Woodruff (2014). Microenterprise growth and the flypaper effect: Evidence from a randomized experiment in ghana. *Journal of Development*

Economics 106, 211 – 226.

Fafchamps, M. and C. Woodruff (2017). Identifying gazelles: Expert panels vs. surveys as a means to identify firms with rapid growth potential. *World Bank Economic Review 31*(3), 670–686.

Field, E. (2007). Entitled to work: Urban property rights and labor supply in peru. *The Quarterly Journal of Economics 122*(4), 1561–1602.

Filmer, D. and N. Schady (2011). Does more cash in conditional cash transfer programs always lead to larger impacts on school attendance? *Journal of Development Economics 96*(1), 150 – 157.

Fiszbein, A., N. Schady, F. H. Ferreira, M. Grosh, N. Keleher, P. Olinto, and E. Skoufias (2009). *Conditional Cash Transfers : Reducing Present and Future Poverty*. Number 2597 in World Bank Publications. The World Bank.

Foster, A. D. and M. R. Rosenzweig (1996). Comparative advantage, information and the allocation of workers to tasks: Evidence from an agricultural labour market. *The Review of Economic Studies 63*(3), 347–374.

Gandhi, A., S. Navarro, and D. Rivers (2016). On the Identification of Production Functions: How Heterogeneous is Productivity? Technical report.

Gertler, P. J., S. W. Martinez, and M. Rubio-Codina (2012, January). Investing cash transfers to raise long-term living standards. *American Economic Journal: Applied Economics 4*(1), 164–92.

Gollin, D. (2014, August). The lewis model: A 60-year retrospective. *Journal of Economic Perspectives 28*(3), 71–88.

Government of Thailand (2004). Act on national village and urban community fund (b.e. 2547). *Royal Thai Government Gazette 59*(9), 442–455.

Greaney, B. P., J. P. Kaboski, and E. V. Leemput (2016). Can Self-Help Groups Really Be "Self-Help"? *Review of Economic Studies 83*(4), 1614–1644.

Handel, B. and J. Schwartzstein (2018, February). Frictions or mental gaps: What's behind the information we (don't) use and when do we care? *Journal of Economic Perspectives 32*(1), 155–78.

Haselmann, R., D. Schoenherr, and V. Vig (2017). Rent-seeking in elite networks. Technical report, Forthcoming, Journal of Political Economy.

Haughton, J., S. R. Khandker, and P. Rukumnuaykit (2014). Microcredit on a large scale: Appraising the thailand village fund. *Asian Economic Journal 28*(4), 363–388.

Hoffmann, V., V. Rao, V. Surendra, and U. Datta (2017). Relief from usury: Impact of a community-based microcredit program in rural india. Technical Report 8021, The World Bank.

Hoynes, H. W. (1996). Welfare transfers in two-parent families: Labor supply and welfare participation under afdc-up. *Econometrica 64*(2), 295–332.

Hoynes, H. W. and D. W. Schanzenbach (2012). Work incentives and the food stamp program. *Journal of Public Economics 96*(12), 151 – 162.

Hussam, R., N. Rigol, and B. Roth (2017). Targeting High Ability Entrepreneurs Using Community Information: Mechanism Design In The Field. Technical report.

Kaboski, J. P. and R. M. Townsend (2005). Policies and impact: An analysis of village-level microfinance institutions. *Journal of the European Economic Association 3*(1), 1–50.

Kaboski, J. P. and R. M. Townsend (2011). A structural evaluation of a large-scale quasi-experimental microfinance initiative. *Econometrica 79*(5), 1357–1406.

Kaboski, J. P. and R. M. Townsend (2012, April). The impact of credit on village economies. *American Economic Journal: Applied Economics 4*(2), 98–133.

Karlan, D. and J. Morduch (2010). Access to finance. *Handbook of Development Economics 5*, 4703 – 4784. Handbooks in Economics.

Karlan, D., B. Savonitto, B. Thuysbaert, and C. Udry (2017). Impact of savings groups on the lives of the poor. *Proceedings of the National Academy of Sciences 114*(12), 3079–3084.

Karlan, D. and J. Zinman (2010). Expanding credit access: Using randomized supply decisions to estimate the impacts. *Review of Financial Studies 23*(1), 433–464.

Khwaja, A. I. and A. Mian (2005). Do lenders favor politically connected firms? rent provision in an emerging financial market. *The Quarterly Journal of Economics 120*(4), 1371–1411.

Kinnan, C., K. Samphantharak, R. Townsend, and D. Vera-Cossio (2017). Village Networks and Household Finance in Village Economies. Working paper.

Kinnan, C. and R. Townsend (2012). Kinship and financial networks, formal financial access, and risk reduction. *The American Economic Review*, 289–293.

Kraay, A. and D. McKenzie (2014). Do poverty traps exist? assessing the evidence. *Journal of*

Economic Perspectives 28(3), 127–48.

Kremer, M., J. Lee, and J. Robinson (2010). The return to capital for small retailers in kenya: Evidence from inventories1. *Mimeo Harvard University*.

Ksoll, C., H. B. Liller, J. H. Lnborg, and O. D. Rasmussen (2016). Impact of village savings and loan associations: Evidence from a cluster randomized trial. *Journal of Development Economics 120*(Supplement C), 70 – 85.

Kydland, F. E. and E. C. Prescott (1982). Time to build and aggregate fluctuations. *Econometrica 50*(6), 1345–1370.

Levinsohn, J. and A. Petrin (2003). Estimating production functions using inputs to control for unobservables. *The Review of Economic Studies 70*(2), 317.

Lewis, W. A. (1954). Economic development with unlimited supplies of labour. *The Manchester School 22*(2), 139–191.

Mabry, B. D. (1979). Peasant economic behaviour in thailand. *Journal of Southeast Asian Studies 10*(2), 400–419.

Mansuri, G. and V. Rao (2004). Community-based and -driven development: A critical review. *The World Bank Research Observer 19*(1), 1–39.

McKenzie, D. (2017). Identifying and spurring high-growth entrepreneurship: experimental evidence from a business plan competition. *American Economic Review 107*(8), 2278–2307.

McKenzie, D. and C. Woodruff (2008). Experimental evidence on returns to capital and access to finance in mexico. *World Bank Economic Review 22*(3), 457–482.

Mckenzie, D. J. and D. Sansone (2017, December). Man vs. machine in predicting successful entrepreneurs : evidence from a business plan competition in Nigeria. Policy Research Working Paper Series 8271, The World Bank.

Meager, R. (2016). Understanding the impact of microcredit expansions: A bayesian hierarchical analysis of 7 randomised experiments. Technical report.

Menkhoff, L. and O. Rungruxsirivorn (2011). Do village funds improve access to finance? evidence from thailand. *World Development 39*(1), 110 – 122.

Moerman, M. (1969). A thai village headman as a synaptic leader. *The Journal of Asian Studies 28*(3), 535–549.

Morduch, J. (1999, December). The microfinance promise. *Journal of Economic Literature 37*(4), 1569–1614.

Muralidharan, K., P. Niehaus, and S. Sukhtankar (2017). General Equilibrium Effects of (Improving) Public Employment Programs: Experimental Evidence from India. Technical report, University of California, San Diego.

Nyshadham, A. (2014). Learning about comparative advantage in entrepreneurship: Evidence from thailand. Working paper, Boston College.

Olley, G. S. and A. Pakes (1996). The dynamics of productivity in the telecommunications equipment industry. *Econometrica 64*(6), 1263–1297.

Phongpaichit, P. and C. J. Baker (2004). *Thaksin : the business of politics in Thailand / Pasuk Phongpaichit, Chris baker* (1st ed. ed.). Silkworm Books Bangkok.

Samphantharak, K. and R. M. Townsend (2010, December). *Households as Corporate Firms*. Number 9780521195829 in Cambridge Books. Cambridge University Press.

Schreiner, M. (2000). Credit scoring for microfinance: Can it work? *Journal of Microfinance 2*(2).

Shaban, R. A. (1987). Testing between competing models of sharecropping. *Journal of Political Economy 95*(5), 893–920.

Shenoy, A. (2017). Estimating the production function when firms are constrained. Working paper, University of California, Santa Cruz.

Shultz, P. T. (2004, June). School subsidies for the poor: evaluating the Mexican Progresa poverty program. *Journal of Development Economics 74*(1), 199–250.

Skoufias, E. and V. D. Maro (2008). Conditional cash transfers, adult work incentives, and poverty. *The Journal of Development Studies 44*(7), 935–960.

Tarozzi, A., J. Desai, and K. Johnson (2015, January). The impacts of microcredit: Evidence from ethiopia. *American Economic Journal: Applied Economics 7*(1), 54–89.

Townsend, R. M. (2007a). Townsend Thai project household annual resurvey, 1998-2006. [Computer file]. 2nd Release. Chicago, IL: Social Sciences Computing Services, the University of Chicago [Producer & distributor].

Townsend, R. M. (2007b). Townsend Thai project initial household survey, 1997. [Computer file]. 2nd Release. Chicago, IL: Social Sciences Computing Services, the University of Chicago [Producer & distributor].

Townsend, R. M. (2014). Townsend thai project monthly survey (1-172) initial release.

Vera-Cossio, D. (2018). Targeting credit through community members. Working paper, University of California, San Diego.

World Bank (2008). *Finance for All? Policies and Pitfalls in Expanding Access.* World Bank Policy Research Report. World Bank.

Zambrana, O. M., G. Z. Cruz, F. A. Velasco, A. C. Callisaya, and E. M. Idiquez (2004). *LA EDUCACIN EN BOLIVIA INDICADORES, CIFRAS Y RESULTADOS* (2nd ed.). Ministerio de Educacin Bolivia.